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PLATE I.

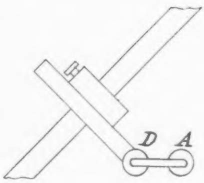


Fig. 1

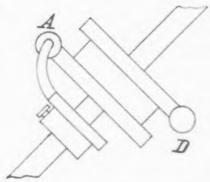
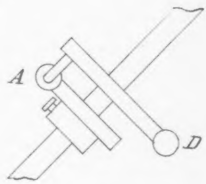


Fig. 2



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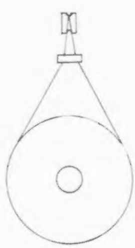

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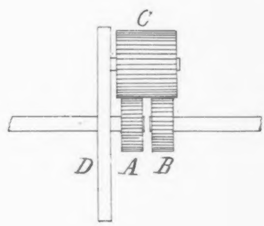

Fig. 5

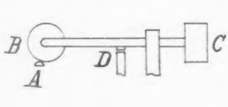

Fig. 6

Fig. 7

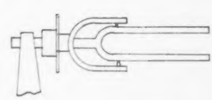

Fig. 8

TELESCOPE MOUNTINGS AND DOMES.

ASTRONOMY AND ASTRO-PHYSICS, JANUARY, 1894.

Astronomy and Astro-Physics.

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General Astronomy.

TELESCOPE MOUNTINGS AND DOMES.*

WILLIAM H. PICKERING.

A recent brief visit to Europe has enabled me to make a study of a number of different telescope mountings in use in England and France, and to compare them with some of those employed in our own country. An especial study has been made of the arrangements for following a star in the telescope, and keeping it exactly upon the cross hairs. This is a matter of comparatively little consequence for visual work, but for photography it is one of vital importance, since owing to variations in the refraction with the altitude of the star, the driving clock can never follow with perfect accuracy, no matter how accurately the clock itself may run.

By far the most common method of following is by means of a slow motion screw adjusted by hand. This works in a nut in most of the American telescopes, while in the foreign instruments it usually fits into a worm wheel or sector similar to that actuated by the driving screw. In the Clark mounting, Figure 1, the driving screw D is attached to a nut working on the slow motion screw A, both being seen endwise. By turning the slow motion screw the driving screw is moved bodily forwards or backwards, thus turning the worm gear which is clamped to the polar axle. In the Warner & Swasey mounting which is somewhat similar in construction to that represented in Figure 3, the driving screw turns a worm gear which is loose on the polar axis. This worm gear carries the slow motion screw which fits into a nut attached to an arm which is clamped upon the polar axle. In the French method, Figure 2, as made by Gautier, there are two worm gears exactly alike, cut upon the same wheel, which turns loosely upon the polar axle. The slow motion screw A is clamped to the axle and is carried by the loose wheel, which is driven by the driving screw D. In the English mounting as

* Communicated by the author.

made by Grubb, Figure 3, the slow motion screw is attached to the loose wheel, and this screw turns a second worm gear which is clamped to the polar axle. Grubb usually employs sectors instead of complete wheels, as do also the other makers very frequently.

For photographic work the complete wheel for both driving screw and slow motion has a decided advantage over the sector, as the latter rarely permits of exposures of much longer than two hours' duration. Where several short exposures are to be made in one evening it is a great inconvenience to be constantly obliged to set back the sector, or else to have to consider whether it is likely to run out before the exposure is over. The only advantage of the sector is that it allows a greater radius to the worm gear than does the wheel, but if the gearing of the wheel is carefully ground, no appreciable irregularity in the motion will be introduced by this cause. While both of the European methods employ an extra worm gear for the slow motion they have the corresponding advantage where a complete wheel is used that neither of the screws ever come to an end, but may be turned indefinitely. It seems to me therefore that a complete worm gear for each screw is most desirable, and that here if we use a slow motion screw at all, we cannot do better than follow the European example.

The method of turning the slow motion screw is usually either by a double Hooke's joint, or by a cord passing over a large wooden wheel. In the latter case the cord may be conveniently made to embrace the wheel by passing it through a small but heavy ring which hangs freely upon the cord by its own weight, Figure 4, and thence over a horizontal axis.* The Henry brothers use the Hooke's joint, with a long handle capable of reaching the hand when the observer is looking through the finder. We have employed the same method at Harvard, but have found it inconvenient and rather unsatisfactory. For very large telescopes it is almost out of the question. Besides the cord, and the Hooke's joint, there is a third method of turning the slow motion screw, employed first by Repsold. This is the method of ring gears, in which the screw is connected directly by a system of mitre gears turning upon the polar and declination axles with a long handle running down the side of the telescope tube. Thus the declination and the right ascension adjustments are made by turning two handles placed side by side. This is a very convenient arrangement, but there is a certain amount of

* This arrangement is used in Dr. Huggins' Observatory.

back lash introduced by this adjustment, and for large instruments the ring gears are necessarily very heavy. Still another method of turning the slow motion screw which has occurred to me is to connect it through suitable gearing with a small electric motor. Wires could then be attached by which the current could be transmitted in either direction through the motor, causing it if properly arranged, to carry the screw either backwards or forwards, at will. This seems in some respects the most completely controlled method of turning the screw in case electric power is at hand.

But there are other methods of correcting the motion in right ascension which dispense with the slow motion screw altogether. One of these, known as the "mouse control," is employed by Grubb on many of his instruments. In Figure 5 let A and B be two gears which are just alike save that one of them has one more tooth than the other. Their respective axles are in line but disconnected. The left hand axle transmits the power, and the right hand one is connected with the driving screw. D is a loose pulley, carrying the loose gear or so-called "mouse," C, which connects the gears A and B. When the power shaft revolves, carrying the pulley D, the driving screw shaft also turns at the same rate. If now we stop D in any position, the differential gearing of A and B comes into play, causing the driving screw to move at a slightly increased or diminished speed as the case may be. D may either be turned backwards or forwards by a cord, increasing or diminishing the rotation of B, or else two separate mouse controls may be employed, one for increasing and the other for diminishing the speed. In the latter case the pulleys are stopped electrically by wires leading directly to the hand.

It seems to me that the "mouse control" is a decided step in advance in the direction of accurate adjustment, over the slow motion screw. In our 13-inch telescope an endwise push of the driving screw of $\frac{1}{6000}$ of an inch moves the photographic plate through $1''$. This slight motion of the screw may be entirely masked by the back-lash, or other causes, and in practice we find such is the case, and that accurate following for enlargments is a very difficult matter. The mouse control avoids all this, by simply changing the rate of motion of the driving screw, in the simplest manner imaginable, and no slow motion screw whatever is needed.

There is a very convenient device of general application which is employed at Greenwich, but whose bearing in the present instance does not at first sight appear. When it is wished to

correct the error of the standard meantime clock, it is not done by adding or removing small weights from a scale pan, as is customary at many observatories. Instead of this there is attached to the front of the pendulum of the clock a verticle permanent bar magnet, measuring 5 × 3/8 × 1/8 inches. Just below the magnet is placed a helix attached to the clock case. It is connected with a battery of eight Leclanché cells, and has no iron core. By passing a current through this helix in one direction or the other the error of the clock can be changed by six seconds, inside of one hour. In talking this matter over with Mr. Douglass afterwards it appeared that this arrangement might be adapted to the control clock of an equatorial. It would probably be best in order to change the rate more rapidly to employ two helices, one above and one below the magnet, and to furnish them with iron cores. By this arrangement no slow motion screw and no "mouse control" would be necessary. An electric current would merely be passed through the helices the intensity and direction of which could be varied by turning a switch held in the hand. Like the "mouse control" this adjustment would never come to an end, and it would have an advantage even over it in one respect. If in following a star we found that it was gradually leaving the crosswires, we should not be obliged to continually bring it back again every few minutes, as soon as we were able to detect an appreciable error in its position. We should do better than that. We should pass a slight current through the coils and change the rate of the clock, so that no error of position should occur,—and this without disturbing the driving mechanism in the slightest degree. It seems to me that we have here a new principle of following,—instead of constantly correcting the error of the clock, as the star varies in altitude, we merely correct the clock rate.

At Greenwich two methods of correcting the equatorial are employed, a rapid one by the slow motion screw, and a slow one by the "mouse control." I should suggest a rapid correction by a quick acting "mouse control" run forwards or backwards by a small electric motor, and a slow correction by the clock, and a rejection of the slow motion screw altogether.

Leaving this subject, let us now turn to the automatic control. This control is of two kinds; first that which is permanently attached to the driving clock, and is in continuous action though at a varying rate, throwing additional work on the clock when it runs too fast, and relieving it when it runs too slowly. The second kind is that which acts intermittently, throwing in addition-

al work when the driving clock gets ahead of an independent control clock, but never relieving the driving clock of any of its own work. In the Grubb and Warner & Swasey mountings the continuous control is accomplished by means of a ball governor, which, when the balls fly out, increase the pressure and therefore the friction, upon a fixed smooth metallic surface. In the Clark mounting the continuous control consists of a rapidly revolving fan wheel, whose vanes are set at a fixed angle before starting the clock. In the Gautier mounting invented by Foucault, the balls of the ball governor are each set in the center of a fan, so that when the speed is increased the fans fly out, and a very large increase of atmospheric resistance ensues, which regulates the rate of the clock in a surprisingly perfect manner. This form of control has been employed upon some American chronographs. So perfect, indeed, in action is it when carefully made, that Gautier does not consider it necessary to employ any additional intermittent control, or any pendulum whatever about the telescope. Nevertheless the astronomers who have used it tell me that while the clock follows admirably for short exposures such as five minutes, as indeed I was able to prove to my own satisfaction, yet when the exposure is longer, slight corrections were constantly necessary. These deviations may of course have been produced by refraction, but the observers themselves seemed to think that the introduction of a pendulum into the mechanism would be an improvement. The Foucault adjustable vanes are it seems to me an improvement on the fixed vane system, and I am inclined to think would be more uniform in their action than any control depending on the friction of solid bodies. In connection with a "mouse control" this device seems to me admirably adapted to telescopes intended solely for visual purposes.

Regarding the intermittent control introduced by an external and independent clock, Gautier, as we have seen, employs none. Grubb again introduces mechanical friction, but in the Greenwich instrument substitutes as omewhat complicated form of "mouse control," while Clark permits a spiral spring to be wound up to a greater or less extent, which when electrically released at the end of each second delivers its absorbed energy in the form of a blow, which does not affect the speed of the driving mechanism. When the parts affected are made strong enough to withstand the repeated blows, the latter method gives very excellent results.

Regarding the electrical connections, I found little to be learned in Europe. The Greenwich method of closing the circuit was to let the end of the pendulum swing through a drop of mercury. This

method enjoys the advantage of simplicity, but in our own case we found that the mercury had constantly to be renewed, at some inconvenience, and that even then the action was not always certain. A second arrangement that we tried consisted of a pin inserted on the face of the pendulum rod, near the top, which at each swing of the pendulum lifted a light spring furnished with a platinum contact. This is a well known method, but we found that unless the clock is a strong one, it is liable to stop it. We next tried attaching a small permanent horseshoe maget to the face of the pendulum, and allowed it to swing over an armature attached to a spring. This device is also an old one but was described anew a few years ago in the *Sidereal Messenger* by Mr. Gerrish. It works pretty well, but though there is no friction introduced whatever, there is a much larger amount of energy absorbed in the magnetic induction than one would at first sight by any means suppose. The next apparatus for the purpose that we tried was made for us by a Peruvian watch-maker, and is represented in Figure 6. A pin A attached to the pendulum lifts a little lever BC by passing under the loose wheel B. This breaks the circuit at D. This contact is easily reached and cleaned without disturbing the pendulum, and the apparatus furnishes on the whole the best make and break that we have yet found.

In bringing this article to a close I will take advantage of this opportunity to make a few suggestions that have occurred to me as a result of my investigations. They are particularly adapted to the mounting of a large telescope, but some of them may perhaps be of use in the case of a smaller instrument. It seems to me that the cheapest and lightest form of building that could be erected to cover a large telescope would be built upon the plan of a large railway depot. In Figure 7 which is a plan of the dome, a steel ring is first laid to form its base. On this a series of parallel steel arches are erected of suitable sizes. These are united and braced by a series of cross ties giving the structure its final form. For small domes wood could be used as a substitute for steel. The whole is then either covered with sheet metal, or perhaps better still with galvanized iron netting. Finally the netting is covered with water proofed canvas. It has been found at Greenwich that netting covered with papiermaché stands the climate very well, and we have found in Cambridge that the same is true of canvas stretched upon wood. It therefore seems that a covering such as is here suggested might withstand very well even the rigors of a northern climate. The shutters of the dome

should slide horizontally according to the Hough or Warner, Swasey plans. The dome should be mounted upon a cylindrical iron framework, covered with one thickness of sheet iron. It is almost universal in Europe to mount the domes upon a heavy base of masonry, which collects and retains the heat well into the night. This principle seems to me to be radically wrong. We have gone to the opposite extreme at Harvard, the buildings, supporting the later domes consisting of a light wooden framework covered with a single layer of thin sheathing. Our experience there and particularly at Arequipa I think fully justifies our method of reasoning.

Permanently attached to the side of the dome, opposite the shutter, should be placed the curved rails on which the chair is to be raised and lowered. The chair, so-called, is a large structure, more nearly comparable to a small room, or perhaps better still to a theatre box. It is furnished with an invalid's reclining rolling chair with book rack attached for making observations near the zenith, with several ordinary chairs, a case of drawers for eyepieces, etc., a table and book shelves for a recorder, mean and sidereal clock dials controlled electrically, and with suitable sounders which can be thrown into the circuit when desired. The motions of the dome, shutter, and chair, which latter is partly counterbalanced by weights moving upon the opposite portion of the dome, are all necessarily moved by an electric-motor, which may be controlled from the chair. The telescope would be clamped in right ascension by the pressure between an electro-magnet and its armature. The clamp could thus be instantaneously applied.

The telescope itself is not run by clock-work, but by a small motor. Since even a large telescope can be so balanced as to be readily moved with one hand, it is evident that only a very small motor would be required for this work, one for instance such as might be employed to run an ordinary sewing machine. Such a motor would hardly need a separate pier from the telescope, but if found best one could be provided for it. The speed of the motor would be regulated by a tuning fork commutator, which would be more than sufficiently uniform for the purpose, (Figure 8). The rate of vibration would be regulated by jaws pressing upon the prongs of the fork near the crotch, and the length of the prongs thus regulated by a micrometer screw. The tuning fork would be placed in the chair beside the observer, who could allow for the varying refraction of the star he was observing by a touch of the screw. This plan would have all the advantages of

the pendulum control above described, since the observer would change the rate, and not the error, of his driving mechanism. In short the motor would save expense, save space and save winding. There would be no slow motion screw, no pendulum, no revolving fans, no ball governor. Yet it seems as if it would give quite as good results as the driving clock, besides being far more easily and conveniently controlled.

HARVARD COLLEGE OBSERVATORY,
December 1, 1893.

THE NATIONAL ARGENTINE OBSERVATORY.*

JOHN M. THOME, DIRECTOR.

The National Observatory of the Argentine Republic was created by an act of Congress in the year 1869; it owes its inception, however, to the efforts of an American Astronomer, Dr. B. A. Gould, of Cambridge. It was the life-long desire and purpose of this distinguished scientist to carry out a systematic survey of the southern heavens between the parallels of 23° and 80°, and to this end he had already accumulated a fund and acquired an instrumental equipment sufficient to warrant his undertaking, when his project became known to the minister of the Argentine Republic in this country, D. Domingo F. Sarmiento,—then a candidate for the presidency of that nation,—who embraced the idea with enthusiasm, and offered his powerful aid toward equiping the expedition in a more complete manner with the object of establishing it as a permanent national institution.

Immediately after his election to the executive chair, President Sarmiento procured the passage of the act already mentioned, accompanied by a modest appropriation for the purchase of instruments and the erection of a suitable building to contain them. Dr. Gould was at once appointed director of the new institution, and was authorized to nominate four assistant astronomers. I had the good fortune to be selected as one of the assistants, and arrived in Cordoba with my colleagues about the middle of September of 1870.

The instruments purchased by Dr. Gould for the Observatory, and which still comprise its equipment, were a meridian circle with 4½-inch object-glass by the Repsolds, two twelve inch lenses

* Read at the Congress of Astronomy and Astro-Physics, Chicago, August, 1893.

by Fitz of New York, one of them corrected for the photographic rays, two chronographs, a portable equatorial by the Clarks with 4½-inch glass, a comet seeker, Zöllner photometer, a repeating circle with tripod by Pistor and Martins, a sidereal circuit breaking clock by Tiede, various chronometers, barometer, thermometer, etc.

The erection of the buildings was begun immediately after our arrival upon a piece of land ceded for the purpose by the Provincial government. Much delay was encountered owing to the difficulty of obtaining skilled workmen; and the installation of the meridian circle was not effected before the middle of the year 1872, owing to a blockade of the ship during the French and German war.

The Observatory building is 100 feet in width by 80 in length, and terminates in domes, of which the east and west ones are 18 feet in diameter, and are connected with the main building by meridian rooms. The two smaller ones on the north and south are situated directly over the corresponding entrances. Adjacent to the Observatory on the east and west are the dwellings of the director and assistants, respectively, and there are some four acres of land in the enclosure, which is now under irrigation. The situation is in the immediate neighborhood of the city of Cordoba, on the south margin of the valley in which the city lies, and distant one mile from the central plaza. Its elevation above this is 100 feet, and above mean tide in Buenos Aires 413 meters. On the west, at a distance of about ten miles, a range of mountains, whose mean elevation above the plain is 1800 feet, extends in a nearly north and south line, and distant some fifty miles more, there is a parallel range, with truncated summit, forming a nearly level plateau several miles in width, whose mean elevation above the sea is nearly 6,000 feet. To the north, east and south, there extends an unbroken plain, almost treeless and waterless, where the drouth is sometimes prolonged over six months at a time. It has happened, various times, that the river Primero, which is the natural outlet for all the water in the valleys between the mountain ranges which I have mentioned, has run dry immediately below the city.

It was owing to these conditions—a dry transparent atmosphere, and a generally cloudless sky—that Cordoba was selected as the site for the Observatory. It is besides, nearly centrally situated with respect to the boundaries of the Republic, being distant 23^m 19^s from Buenos Aires on the east and less than 20^m from Mendoza on the west. The climate is in general, exceed-

ingly pleasant, being equally free from the sultriness of the summer nights in these latitudes, and the extreme rigor of your long and tedious winters, with their consequent discomforts to the working astronomer. It is a nearly ideal location for an Observatory, in short, and the promise made by President Sarmiento and his Congress, to protect and maintain the institution, has never been broken nor ignored by any of their successors under whatever stress of circumstances. My long residence in the country has been invariably agreeable and pleasant, and my intercourse with the citizens of all classes in different parts of the Republic, enables me to testify to their many noble, generous and brilliant qualities.

During the period embraced between our arrival in Cordoba and the mounting of the Meridian Circle, our energies were directed towards the elaboration of a Uranometry which should contain the true magnitudes and positions of every star as bright as 7.0 mag. in the region of sky extending from the south pole to ten degrees north of the equator. The estimates of magnitude were based upon the scale employed by Argelander for his Uranometry of the northern heavens, in order that the results should be directly comparable, but our final magnitudes resulted from the mean of at least two independent estimates of the same star by different observers upon a scale of tenths. Every part of the sky was examined twice at least for identifications and magnitudes by different observers, and where the estimates of magnitude differed considerably, new ones were made. Great care, entailing additional labor, was necessary for the correct identification of the stars in or near the clusters, or where the magnitudes as given in the existing catalogues were erroneous, and considerable discordance arose between the estimates of the various observers in the cases of colored stars; but after we had gained the proper skill by persistent practice, the average difference of our estimates did not exceed two-tenths of a magnitude for the white stars. After the stars had all been platted upon the final charts, I again compared them directly with the sky, for possible errors of identification or omissions, and obtained new estimates of magnitude where these were desirable. The intimate acquaintance with the southern constellations which I acquired in this way has always remained one of the chief pleasures of my life.

Argelander's Uranometry represents the utmost reach of his vision, and I believe he prided himself on the fact that all his estimates of magnitude were made by his unaided eye. In the Uranometria Argentina, the lowest limit is a fixed round magnitude,

faintly visible on a clear night to a good eye, but not based upon any particular one, and our estimates for the fainter stars were made with the aid of an opera glass, necessarily. It would have been impossible otherwise, to make an exact alignment. The Urancmetria Argentina also gives the first attempt at bounding the constellations by meridians and parallels; or, in other words, giving them boundaries that can be exactly described and located. The work, although complete in its essential features by the end of 1872, was not entirely satisfactory until the year 1877, when I brought the maps to New York to be printed.

With the installation of the meridian circle in 1872, preparations were at once made for the great undertaking of the Observatory. At the time of our advent in the southern hemisphere, there was neither a trustworthy Uranometry nor catalogue of stellar positions that could lay claim to completeness and exactness in existence. Such catalogues as had been published were distributed almost at random among the brighter stars of that immense region. On the northern limit Argelander had pushed his zones across the low altitudes to the parallel of 30°, and the region lying within 20° of the south pole had been observed by Gillis in Santiago. The intermediate region was occupied by catalogues, which were either of a partial or defective nature, and of epochs ranging from Lacaille's, in 1750 to Ellery's excellent observations at Melbourne, but none of them gave even on approximate idea of what might be beyond the limit of visibility, and they were all totally inadequate for the requirements of determining the course of any movable body. Until the completion of our zone observations we were compelled, when observing planets and comets, to use anonymous stars for the larger part of our comparisons, and this still holds true, though in less degree, when we pass to the north of the limit of the Cordoba Zones. Within that limit, however, the requirement of a conveniently situated and well determined point of comparison for any given moment is now very nearly met; certainly in the large majority of cases.

At the express desire of Argelander, I believe, Dr. Gould began his zone observations with the parellel of 23°, and then gradually proceeded, by convenient distances in declination, with generous laps, to the northern limit of Gillis's circumpolar work. The duration of the zones averaged nearly two hours, and there were three of these observed upon every clear night. An indispensable requirement at the beginning and end of each night's work and during the intervals of rest by Dr. Gould between zones, was the rigorous determination of all the instrumental and clock correc-

tions. At the same time, such stars as were conveniently situated for observation during these intervals were included in the regular programme of the night's work. These were observed over eleven threads, all the four microscopes were read for the declination co-ordinate. The observations were made by two assistants detailed for the night, who alternated with the zones.

Only one microscope was read for the zone observations, and only one reading of the microscope was made for each star, but this was afterwards referred to the mean of all the four microscopes, which were carefully read for both north and south limit at the beginning and end of each zone. The transits were usually over one tally only, consisting of either three or five threads, but in the laps, or where it seemed desirable to re-observe a zone, many observations were duplicated and some stars have been observed as many as five times. All the observations at the telescope—transits, declinations and magnitudes—were made by Dr. Gould; the assistant made the microscope bisections and recorded them, the magnitudes, name of tally employed and approximate time of transit. The transits were made at any convenient part of the field, but the settings in declination were always referred to the zero of the two fixed horizontal threads. The transits were recorded directly upon the chronograph by the observer, and four complete records of as many stars could be made in a minute when the stars were at all evenly distributed.

By this method, each star was as independently observed as though it constituted the entire zone, and in all the various reductions up to the final one of mean epoch, it was so treated. Consequently, the only error affecting the adopted places is the inevitable one of observation, as affected by blurred or unsteady images or fatigue of the observer's eye. Taken as a whole, and considering the nature of the work, they seem to me to be a remarkably fine series. The zones were finished in 1875. More than 100,000 complete observations had been made in that time.

Growing out of the small beginnings between zones and the partially clouded nights, when some observations were impracticable, and advancing to full swing when these were completed, arose the structure of our General Catalogue of rigorous positions, which was finished in 1880. The observations and reductions for this catalogue were made, almost exclusively, by the assistant astronomers, and every operation was performed with all the care and deliberation required in fundamental work. In the beginning we employed three minutes of time and the services of two assistants to record a complete observation, *i. e.*: 11

transits for the catalogue and circumpolar stars, 17 for the time stars, and four microscope readings. As the work proceeded, this interval was reduced to two minutes. which was found to be amply sufficient to insure accurate work, Our working list of stars, which was collected by Dr. Gould, contained: 1st, all the visible, Uranometry, stars; 2d, fainter stars from the published catalogues; 3d, anomyous stars which had been noticed during the course of the work, and finally, Zone stars. It is, therefore, a General Catalogue in the literal sense of the word, because it includes all the stars contained in every southern catalogue except the Cordoba Zones. The number of complete observations made for it exceeded 140,000, beginning with the middle of 1872, and ending with 1880.

Conjointly with the catalogues mentioned, a large number of planets and comets were observed, the geographical co-ordinates of the Observatory and of most of the capitals of the various provinces and the river ports were determined telegraphically, and an extensive series of photographs of the principal stellar clusters and double stars was obtained.

At the beginning of the year 1885, Dr. Gould, who had so admirably and successfully directed the affairs of the Observatory from its beginning, steadfastly guiding its course amid innumerable obstacles, difficulties and personal afflictions toward the accomplishment of the task which he had set for himself, was compelled to yield to the severe strain upon his health, and retired crowned with honors and the laurels of a service to astronomy which has probably never been surpassed by any other man. In his final memorial to the minister, he briefly outlined the course which, in his opinion, it would be most advantageous to follow in the interests of astronomy, and it has been my aim, as his successor, to conform to this as closely as circumstances would permit. Unfortunately the financial crisis, which had not then begun, has since grown into such proportions that we are now compelled to struggle along under a greatly reduced appropriation—nominally the same, but really two-thirds less.

Continuing the observations of comets and planets, of which we have already accumulated several thousands; the determinations of latitude and longitude in various parts of the republic; photometric and photographic work; and various special problems for the solution of which our aid was solicited by the foreign astronomers, our chief aim has always been to explore the region of sky which is distinctively our own, and to this end we have begun the extension of the northern Durchmusterungs from the limit to

which they had been carried by the astronomers at Bonn. Our observations at present number over one million, and give the approximate position and magnitude of every star down to the 10th magnitude in the belt of the heavens comprised between 22° and 42° of south declination, some 340,000 in number. The first volume of these results has been published and distributed, and the second, together with a series of 12 charts covering the region, will be finished about the beginning of the coming year. The unexpectedness of my leave of absence, and the suddenness with which I availed myself of it, interrupted an investigation of the distribution of the stars upon which I was engaged, and here I have had little inclination to take up any work during my short leave after 16 years of continuous duty. I think, however, it can be shown from our results that the law of equable distribution holds good down to our lowest limit.

Meridian observations for rigorous stellar positions have been continued as before, and these now number something over 60,000, all well advanced in the reductions. I have, besides, revised, corrected and published seven volumes of meridian observations in manuscript which Dr. Gould confided to my care, notwithstanding the great falling off in our appropriation. Sixteen volumes, in all, have been published, and there is material ready for five or six more.

I think most astronomers, especially those of maturer age and experience, will agree that our time and means have been well and diligently employed, whatever lack of brilliancy there may be in the achievement. What we have accomplished was done by concentrating our energies and directing them upon one distinctive object. There has never been more than one director at a time in our Institution and the principle seems to have worked well.

PROPER MOTIONS OF DOUBLE STARS.*

S. W. BURNHAM.

I.

There are many double stars, so-called, where the change in the position is obviously the result of proper motion. Strictly speaking these are not double stars so far as the distant components are concerned. Most of these examples are from the cata-

* Communicated by the author.

logues of the Herschels and the Struves. In some of these wide pairs, one or the other of the components has been found in recent years to be a close pair, thus making a real double star, and sometimes a most important physical system. The measures of these distant optical companions, however, serve the purpose of giving a very accurate determination of the relative proper motion. In some instances it is certain that this belongs entirely to the principal star; and in most of the remaining examples it is at least highly probable that this is true of them.

In my observations of new pairs, I have as far as practicable measured an outside star, when there happened to be one near enough for convenient measurement with the micrometer, in order to determine in the future any proper motion that might exist. Many of these stars have already shown unmistakable rectilinear motion, and in some instances the data is sufficient to give the amount and direction of this movement.

I have investigated many of these stars from the various catalogues, and propose to give here some of the results. We already know the proper motions of many optical and physical pairs from positions with the meridian circle, but only stars where the motion is large, and the annual change half a second and upwards. I shall therefore give here as a rule only stars whose proper motions have not been determined from meridian observations. While these quantities in most of the stars are small, and would be very uncertain if the values depended upon positions with the meridian circle, there can be no doubt of the substantial accuracy of the results when based upon measures with the micrometer by experienced observers, and it is not probable that observations in the future will materially change these movements. The graphical method has been used throughout.

No. 1. β 999. ω Andromedæ.

In 1872 I found with the 6-inch that a 9 mag. star in the field with ω Andromedæ had a small attendant; and in 1881 with the 12-inch at Mt. Hamilton I discovered a close companion to the large star. So we have a quadruple group of two pairs separated by a distance of 130″. The measures of the brighter component of the fainter pair commence in 1881, and end in 1891. There has been absolutely no change in the direction of C, but the distance has diminished 3″.3 during this time. From these measures we get 0″.300 in the direction of 110°.3 as the annual proper motion of A. The proper motion given by Stumpe from meridian observations is 0″.348 in 107°.0. I have included

this star in my list because of this change in the value of the proper motion. The close attendant to the principal star certainly shares in this motion. It is therefore a physical system, and certain hereafter to be of special interest. It has already moved 8° in position angle around the other star, the distance remaining constant.

No. 2. β 1101. ψ Cassiopeiæ.

This star has two distant companions, discovered by Herschel in 1783. Struve gave the magnitudes 8.9 and 9.5. The brighter is now 28″ from ψ. These small stars (CD) form a 3″ pair. In 1889 I found a close companion of 13 mag. at a distance of 3″ from the bright star, so that now we have a quadruple group consisting of two pairs 28″ apart.

The measures of C cover a period of something more than a century. The measures of Herschel, Struve, Dembowski, Hall and Burnham give for the annual proper motion of A 0″.063 in the direction of 67°.5. There has been no change in the Struve pair (CD). These stars are relatively fixed, and probably make an optical pair. It is impossible at this time to say positively whether or not the new star has the same proper motion as A. The second set of measures in 1891 showed a direct motion in angle of about 4°, with a little diminution in the distance. This change does not correspond to the proper motion of A, and it is probable that these stars make a binary system.

No. 3. β 487.

The wide pair is Σ 17. The components are 8th and 9th magnitudes, and the distance 27″. B was discovered to be double in 1878, the new companion being very faint, and 2″ from the other. My measures of BC in 1891 show a small increase in the distance, but this may not be real. The measures of the wide pair by Struve, Dembowski and myself give the annual proper motion as 0″.010 in the direction of 209°.4.

No. 4. β 643.

The wide pair is Σ 2342, the principal star being about 6 mag., and the companion nearly 9 mag., at a distance of 29″. In 1878 I found a much nearer and extremely faint companion about 9″ from A. The measures of C from 1830 to 1891 give a proper motion of 0″.052 in the direction of 144°.0. My measures of B in 1878 and 1891 show an apparent displacement of that star, in substantially the same direction as the other, of 0″.043 annually.

As this is a very faint star, and difficult to measure, and the observations cover only a short interval, it is safe to conclude that these companions are relatively fixed, and that the movement is in the bright star. The value and direction of this motion should be taken from the measures of C, with respect to both companions, therefore this star has no further interest as a double star.

No. 5. β 582.

This is another of the wide Struve pairs (Σ 1179). There is no sensible difference in the magnitudes of the components. Struve rated them 8.5 mag., and in my last measures in 1891 I called them 8.7 mag. The distance now is about 20″. In 1878 I found that B was double, having a very minute attendant at a distance of 3″.7. The measures of AB by Struve, Dembowski and Burnham give an annual proper motion of 0″.032 in the direction of 17°.0. As there is apparently no change in BC, the change in the wide pair is due to the motion of A.

No. 6. β 511.

This is very similar to the last named pair. A and B constitute Σ 171, each 8.5 mag. and now about 30″ apart. In 1878 I found a 12 mag. companion 3″.7 from B. The measures of the three observers mentioned in the preceding pair, give for the proper motion of A 0″.042 in the direction of 6°.2. There has been no certain change in BC.

No. 7. β 633. γ Draconis.

This is only an optical pair. The companion is 13 mag. The interval is too short to give as accurate a value of the proper motion as that found for the other stars in this list. Still it must be approximately correct. My measures give 0″.016 for the annual movement in the direction of 283°.0.

No. 8. β 825.

The wide pair is Σ 2268, the components being about 20″ apart. In 1881 I found a new companion between these stars. The change in the Struve star, as would be expected, is clearly due to proper motion. From the measures of Struve, Mädler, Dembowski, and three sets of my own measures from 1881 to 1891, I find the annual displacement of A is 0″.050 in the direction of 345°.9. The measures of B, which cover a period of ten years, do not appear to indicate any sensible change with refer-

ence to A, but the new star is only 13 mag. and such an annual motion might not be apparent in so short a time. If these stars are relatively fixed, it would tend to show that the proper motion was in C rather than A.

No. 9. β 600.

The wide pair was first noted by Sir William Herschel, and it was measured by South and Herschel in 1823, the stars then being 67″ apart. In 1878, the 18½-inch showed a close component to the principal star, which appears to have considerable angular motion. The only measure of the distant star, since those mentioned above, were made in 1878 and 1892, by the writer. These observations show an annual proper motion of 0″.016 in the direction of 99°.0. The principal star is just visible to the naked eye.

No. 10. β 815.

The principal star is nearly 8½ magnitude, and the companion 10½, at a distance of 6″.4 at the time of discovery. The relative motion is clearly rectilinear, and the value should be accurately known. The measures from 1881 to 1892 give an annual movement of 0″.15 in the direction of 142°.9.

No. 11. β 497.

This is a distant companion to a sixth magnitude star (B .A. C. 239). As the close double companion is comparatively a faint star, the motion probably belongs to A, which is 120″ from BC. My own measures and those of Engelhardt give for the annual motion 0″.206 in the direction of 330°.5.

No. 12. H. V 18. α Cassiopeiæ.

There are two companions nearer than that noted by Herschel in 1781. The nearest is 17″ distant, but the only measures are those made at Mt. Hamilton in 1889. For the distant star the measures extend over a period of more than a century. Herschel's positions seems to be substantially correct. The proper motion is, therefore, 0″.093 in the direction of 128°.8.

No. 13. Σ 23.

The principal star is 7.6 mag. and the companion 9.9 mag. When Struve first measured this pair, the distance was nearly 14″. It is now but little more than 6″. The best measures are

very accordant, and give a proper motion of 0″.114 in the direction of 14°.4.

No. 14. Σ 44.

The magnitudes of these stars according to Struve are 8.3 and 9.0. At the time of the first measures, they were nearly 8″ apart. Some of the observations are discordant, but the results found by the best observers show exact rectilinear motion. These measures give for the annual movement 0″.032 in the position-angle of 111°.0.

No. 15. Σ 86.

This is a 12″ pair of small stars, 8.0 and 8.7 mag. The observations are numerous, and the rectilinear motion very decided. These measures give 0″.049 in the direction of 274°.7 as the annual proper motion.

No. 16. Σ 118.

This is a 10″ pair of 8.5 and 9.4 magnitude stars. The measures down to 1888 give a proper motion of 0″.047 in the direction of 310°.3.

No. 17. Σ 133.

The principal star is 7 mag. and has two distant companions which were mentioned by Struve. The nearest at that time was 29″ distant. These stars are respectively 10.5 and 10.8 magnitude. From the measures of Struve, Dembowski and Hall we get for the proper motion of A 0″.019 in the direction of 221°.8. As both companions give exactly the same result, it is certain that the change is all in the principal star.

No. 18. Σ 143.

Struve gives the magnitudes as 7.7 and 9.0. The distance at the time of the first measures was about 30″. The observations of Struve, Dembowski and Burnham give a proper motion of 0″.085 in the direction of 137°.3. There was a time when these stars were only 1″.5 apart.

No. 19. Σ 197.

The measures cover a period of fifty years and are very consistent. From them we find a proper motion of 0″.102 in the position-angle of 50°.6. The respective magnitudes of 7.3 and 8.3.

THE PHOTOGRAPHIC CHART OF THE HEAVENS *

There is a resolution of the Permanent Committee that each co-operating Observatory shall annually report to the Committee the work accomplished during the year. Though as yet no such reports have been published, it is otherwise known that satisfactory progress is being made with the taking of the "Catalogue" plates, that is, the plates of short exposure (6 min., 3 min., and 20 sec.), which will "render possible the construction of a Catalogue." Many Observatories have as yet taken no plates of long exposure (40 min.) at all, and those Observatories which are carrying out the complete scheme have naturally found the number of Catalogue plates increase more quickly than the number of chart plates. It may therefore be hoped that the Catalogue series, at any rate, will soon be completed, although it may be some years before the Chart plates are all taken.

It becomes necessary to consider the reproduction, measurement, and publication of the plates more definitely. Plans already proposed have been criticised somewhat strongly in this magazine on the ground that they were too ambitious. There is a wide distinction between what you would like and what you can get. Perhaps the former might be summed up as follows:—

Reproduction.—Enlarged copies on plate-glass of all the plates to be made by photography and distributed to all Observatories.

Measurement.—The coördinates and magnitudes of all stars on the "Catalogue" plates to be measured several times for all three images.

Reduction.—All the measures to be corrected for the various errors and reduced to R. A. and Decl. 1900.0. Incidentally a large number of meridian observations to be made in order to determine the places of at least five stars on each plate at the epoch 1900.0 to within 1″.

Publication.—All this work to be published in detail, with a final Catalogue of the 2,000,000 stars.

This programme is of course purposely made extravagant; and yet much might be said in favor of all the items. It is a *maximum* programme, and could never be carried out. It has the great merit of simplicity, and any deviation from it immediately raises questions. Such questions are best settled by having a definite issue propounded, and without further preamble (we have lately learned to look upon preambles with suspicion, as contain-

* From *The Observatory*, December, 1893. By the Editors.

ing what is inconvenient to put in the bill) we offer the following programme for criticism. It will be convenient to rearrange the order of the sections as follows:—

Measurement.—If it be admitted that accurate and complete measurement of all stars on the Catalogue plates is impossible, the work may be limited in two directions:

(1) Accurate measurement may be retained for a select number of stars.

(2) All the stars may be measured with a sacrifice of accuracy.

A third course would be to combine the two.

In this article the comparative advantages of the three courses cannot be dwelt upon. We propose the second for the following reasons:

An accuracy about equal to that of the Revised Durchmusterung, which, as M. Loewy has so well pointed out, should be our basis, can be attained with very little labor. The places of the stars may practically be read out by one person and written down by another nearly as fast as he can write. A scale (or scales) in the eyepiece of a microscope divided to 0.1 mm. or 0.05 mm. will enable an observer to call out at sight the place of star to 0.01 mm. or 0.005 mm. by estimation. Or the images might be thrown on a screen and measured with a foot-rule.

Reduction.—Mr. Turner pointed out last month that the rectangular coördinates of a star on a plate are for many purposes more useful than R. A. and Decl.; and has deprecated the waste of labor in reducing such coördinates to R. A. and Decl. unless absolutely necessary.

There is, however, one question to be settled. Each star occurs on two plates, and though its coördinates on one plate are transformable into those on the other by simple formulæ, it remains to be decided how much of this transformation is to be done before publication of the results, and how much is to be left to the future. Now it must be remembered that although the réseau lines of the two plates containing the same star are nearly parallel in the vicinity of the equator, they are sensibly inclined at a distance from the equator; and thus the differences of the rectangular coördinates of a star on two plates may vary by several minutes of arc, according to its position in the the common region. It would lead to great confusion if a star had two sets of coördinates sensibly differing; and hence it would seem reasonable to reduce one set to approximate coincidence with the other by correcting for the sensible inclination of the axis of coördinates, to each other and the distances of the two origins. The actual rule

to be adopted in practice must be to some extent arbitrary, but the following proposition has merits:

The rectangular coördinates of the upper half of each plate to be published as they stand. The coördinates of the lower half to be corrected by those corrections, which would independently of errors of observation and manipulation, bring them into accordance with the coördinants on the two overlapping plates.

Publication.—It is obvious that the real unit is not the whole plate but the quarter plate. A quarter plate contains 13 × 13 squares of $5^{mm} \times 5^{mm}$. Would it be too extravagant to have a form printed on a quarto-page containing 13 × 13 squares, and to express the position of a star by putting *in the corresponding square* numbers expressing its distances (in minutes and decimals) from the top and side? The publication of a Durchmusterung has hitherto been on the plan of limiting the Decl. to a zone of 1°, but allowing R. A. to take precedence within this limit. Some such plan as suggested above is an attempt to distribute the stars by their *two* coördinates instead of *one* only. It has the obvious disadvantage of wasting space in cases where there are only a few stars on the plate, and not being adequate when there are many. But, after all, are we not in the former case already wasting glass and gelatine just in the same way, and does a little paper much matter? And in the latter case, if it is necessary to enter several stars in one box, can we not increase the size of the box?* The great advantage of such a plan would be that it would give at once a general view of the plate; it would, in fact, be half way towards reproduction. (In the lower halves of the plates the boxes would not correspond to the réseau boxes, but would represent a corrected réseau.)

Reproduction.—If the above scheme of publication of measures of the Catalogue plates be adopted, this would be nearly sufficient. Each plate should perhaps be copied at least on glass, and the duplicates from all Observatories deposited at some Central Institution. For the Chart (long exposure) plates similar duplicates should be made; and paper prints would probably be sufficient for distribution.

The above is essentially a *minimum* programme, and practically represents the least that can be done to secure the perman-

* One of the disadvantages of a Durchmusterung of the accepted form is the impossibility of inserting new or omitted stars. The blank boxes of a form such as that here suggested might be utilized in many ways, *e. g.* for the insertion of fainter stars from the Chart plates in barren regions. Such additions might be made either before publication or in MS. after publication. In fact the form would have this and other advantages of an actual map.

ence of the photographic record. If we had satisfactory evidence of the imperishability of the plates, their measurement might be omitted altogether. But gelatine plates have only been known for a small fraction of a century, and we cannot afford to trust to their permanence for one or more centuries. Hence it is advisable, if not imperative, to put in the form of a permanent record *as much as we conveniently can* of the information given by the plates. A most important condition to be observed in settling the limitation italicized is that the work should be completed in about the same number of years as are occupied in taking the plates; otherwise the plates of some part of the sky will be already old before they are measured. Hence an Observatory which takes some hundreds of plates a year ought to be prepared to measure at least a hundred plates a year, and complete measurement is out of the question. On the other hand, some plan of reading out the coördinates and magnitude of a star at sight—about as quickly as they can be written down—would seem to make it possible to get through the enormous amount of work at a proper pace.

Finally such work may be done quite independently of the complete and accurate measurement of a certain number of plates.

The present proposal is very far from being an adverse criticism of the admirable and energetic way in which the plates are being measured at Paris, which will produce results valuable in quite a different way. If any suggestion can with propriety be made as to the scheme of work at Paris, it would be that portions of the sky to be examined in this thorough manner should be chosen in different parts of the sphere, and not confined to a few zones; for in the present article it is assumed throughout that a complete measurement of the whole sphere is impracticable. But results still of the greatest value may be obtained for selected portions of it.

ASTRONOMICAL PUBLICATIONS. I.

WM. W. PAYNE.

It is a question of some interest to the astronomer of the present, how he is to find time to keep himself informed, in regard to what is being done in his own and kindred fields of investigation. He knows that he has not time at command to read and digest even a small part of the useful new publications that appear every month. There are too many of them, and besides that his best strength, mental and physical, must be given to his own

work in some particular line of astronomical investigation. To supply this need is presumed to be the special field of an astronomical journal.

A dozen years ago we began the publication of the SIDEREAL MESSENGER, animated, in the main, with this thought in mind. It was only intended for beginners in astronomy, for that was our own condition in regard to practical work, and we could not presume to instruct professional men in the science who had spent more or less of their lives in original research. Our surprise can hardly be imagined, when after a few months, we found on our subscription books, the names of a considerable number of the most learned astronomers in this country, with some prominent ones from across the Atlantic.

That was a trying place for a novice in the art of publishing to say nothing of the graver responsibilities of editing the MESSENGER, the namesake of that excellent magazine started in July, 1846, by the scholarly and honored O. M. Mitchell while director of the Observatory at Cincinnati. After many severe trials and much useful experience, we found that our venture would not be a failure for lack of support, if all legitimate aid within reach could be kept and gradually increased. In this we were not mistaken for the *Messenger* kept steadily increasing in usefulness for ten years, at the end of which time its size was nearly three times that of the first issue, and its contents improved, at least, in a corresponding ratio. This was more than could have been fully anticipated in view of the discouraging outcome of the attempt of Professor Mitchell,as will be remembered his magazine was discontinued after two years of trial for want of support. That fact in no way places discredit on the noble work of Professor Mitchel, for every one who knows anything about the character of that early magazine very well knows that his self-sacrifizing labor in this direction was not less than in others which may have had wider general notice.

It would be interesting to some of our readers, possibly, if we should give a fuller account of our publications, and also some notices of other similar ones now being published, especially for those who have not ready access to the sources of such information.

We also propose to make a brief review of astronomical journalism in subsequent numbers of this publication, and set out the needs and possibilities of this important field. We desire to say something to our co-laborers which we believe will be of general interest, to the professional astronomer and to the amateur.

Astro-Physics.

THE POLAR RADIATION FROM THE SUN, AND ITS INFLUENCE IN FORMING THE HIGH AND LOW ATMOSPHERIC PRESSURES OF THE UNITED STATES.*

FRANK H. BIGELOW.

In two papers already published (*Amer. Meteor. Journ.*, Sept. 1893, *Astron. and Astro-Phys.*, Oct. 1893), a brief statement has been presented of the lines of evidence that tend to prove the following facts: (1) That the Sun emits two distinct types of radiant energy into the space outside its surface. (2) That the first is propagated radially in all directions, the part falling upon the earth, especially on its equatorial belt, being an electromagnetic wave, whose electromotive force $\int(Xu+Yv+Zw)d\tau$, by the law of conservation of energy, breaks up into the dynamic wave

$$\int\left(u\frac{dF}{dt}+v\frac{dG}{dt}+w\frac{dH}{dt}\right)d\tau,$$

partly inductive and partly magnetic in its instantaneous state, plus the static or potential stress

$$\int\left(u\frac{d\psi}{dx}+v\frac{d\psi}{dy}+w\frac{d\psi}{dz}\right)d\tau,$$

plus the irreversible energy of Joules' Heat

$$\int\frac{u^2+v^2+w^2}{C}d\tau.$$

The electromotive force probably originates in the atomic oscillatory discharges of the ultimate ponderable materials of the photosphere, is propagated with inappreciable loss through the frictionless ether to the atmosphere, where in passing through this gaseous envelope, a series of complex transformations take place. Thus the wave lengths are increased and frequencies are diminished, part of the waves penetrating to the surface of the earth, the remainder transferring their vibrations to the molecular constituents of the air, in the form of heat and of free electricity, which gives rise to the observed electric potential fall. The magnetic wave of induction, accompanying the electric wave in quadrature to it, acts as a vector upon the magnetic lines of force continuously surrounding the earth from its permanent magnetism, and generates a complex system of forces whose to-

* Communicated by the author.

pography has been marked out on my 30-inch globe model. (3) That the Sun also emits a polar magnetic field, emanated from some sort of a rigid nucleus, apparently quite steady in its general manifestations, which comes to the earth, concentrating according to the laws of magnetic conductivity in the oval region embracing the magnetic and geographical poles, though the influence of the same is clearly marked out to about 60° of magnetic polar distance. On the Sun the physical manifestations of this field are found in a heterogeneous distribution of the magnetism, having positive and negative poles, with meridians of greater and less intensity, which control to some extent the location of the sun-spots, the output seen as the corona, and possibly the faculæ and the prominences. At the earth the polar radiation of the Sun is perceived in the spasmodic magnetic storms, the outbursts of visible auroras, and in certain meteorological phenomena. A study of this polar field shows that the sun rotates in 26.68 days synodically, the period appearing persistently, even though masked by a series of overlapping effects, in all the magnetic and meteorological elements heretofore examined. The curves of relative numbers obtained from the study of the residuals in this period, are such as to give much confidence in the physical theories involved. They were derived by massing together many periods, the data embracing all regions of the northern hemisphere, roughly in the preliminary investigation, and were liable to some critical reservations as falling a little short of convincing evidence of the hypothesis. What is lacking is the strictly individual correlation of the solar cause and the terrestrial effect, day by day, and persistently through long ranges of time, such as is seen in gravitational, or in equatorial radiation phenomena. It was not easy to detect the precise way to do this, in the midst of the enormous mass of magnetic and meteorological observations, but such a method has at last been discovered, and the purpose of this paper is to explain the same.

Current Meteorological Theories. Ferrel's analysis of the meteorological phenomena of the atmosphere may be divided into two parts, namely the general motions and the local motions. To obtain the distribution of pressure arising from the general motion, two principles were developed, the first embodying the application of Euler's equations for relative motion, and the second assuming that difference of density arising from difference of temperature is the efficient cause of such motion. His working equations after final transformation have a pressure term on one side, and four terms on the other, expressing inertia, deflec-

tion, friction, and temperature gradient. The comparison of observation and computation assures us that this is substantially the system which exists in nature, although the broad features alone are brought out, the inclusion of details being rather unsatisfactory. It is observed that Ferris formed his equation as a sum of terms, rather than as a product, which the nature of the problem would more plausibly suggest; and that he secured his approximate results by suppressing certain terms as inertia and friction under specified conditions. He was perfectly clear that the motion was due to difference of temperature, and for the general motions took that thermal fall which is observed to exist between the equatorial and the polar regions, arising from the solar equatorial radiation, now called the electro-magnetic radiation. He says in the opening sentence of the chapter on the theory of cyclones, Coast and Geodetic Survey Report, "in the general motions of the atmosphere, the disturbing cause is the difference of density, arising mostly from the difference of temperature between the equatorial and polar regions of the globe." The working out of this idea on the hemisphere fixed in his mind clearly the formation of the polar cyclone surrounded by the tropical anticyclone, the two attended by belts of maximum and minumum pressure with the consequent zones of calms.

The persistence of this mental picture is evident in all the work that followed regarding small or local disturbances, and everything is bent a little, refracted to this point of view. In the next sentence Ferrel says, "in the ordinary cyclonic disturbances of the atmosphere the causes are similar but more local, and consist in a difference of density arising mostly from a difference of temperature between some central area and the external surrounding parts of the atmosphere." This dominant idea has, I believe, proved fatal to Ferrel's successful development of his sound fundamental principles, and has greatly influenced many students to travel a road whose end has never been found. The set of papers in the Coast and Geodetic Survey Report is undoubtedly his most scientific production, for this reason, that he works out his results from the general equations strictly, merely assuming this difference of density between a central area and the region surrounding it. He was at the time evidently unable to satisfactorily account for the energy implied in the temperature difference required to do the work observed in the motions of the cyclones and anticyclones. Many expressions occur to this effect here and there by the way of acknowledging the want of this knowledge, or criticising such theories as were offered to

account for it. For instance, p. 183, "if for any reason there is kept up a continued interchange of air between the central and exterior part"; p. 201, "the condensation of aqueous vapor plays an important part in cyclonic disturbancess but is by no means either a primary or a principal cause of cyclones;" in Waldo's edition of Ferrel's earlier paper, p. 39, "the theory which attributes the whole of the barometrical oscillations to the rarefaction of the atmosphere produced by the condensation of vapor in the formation of clouds and rain cannot be maintained;" on p. 239 C. and G. Report he quotes Loomis as follows, "rainfall is not essential to the formation of areas of low barometer, and is not the principal cause of their formation or of their progressive motion," and remarks "this is strictly in accordance with the theory."

However, being hard pushed to find a cause for his central area temperature in cyclones, he gradually weakened from this position, endorsed Espy's condensation theory of development of latent heat by formation of clouds and rain, and in his last year could write in *Science*, December 19, 1890, "all this has been done in the condensation theory of cyclones, with results so satisfactory as to scarcely leave a doubt as to the truth of the whole theory." This was in reply to Hann's revolt against the sufficiency of this cause to produce the cyclones that were observed, who took the ground that these local gyrations are only subordinate parts in the general circulation which depend upon the effects of equatorial radiation alone, and are independent of any local cause. Hann even went so far as to conclude that "the motion of the atmosphere is not a product of the temperature, but is in spite of it; the temperature is a product of the motion," *Science*, May 30, 1890. Ferrel was loyal to the theory that temperature difference causes the motion always and everywhere, and Hann in adopting the inverse proposition has surely erred against first principles. All this would be a mere expression of opinion on my part, if it were not in my power to indicate an efficient source of the local temperature difference, which is just now the real stumbling block in the way of the advance of Meteorology as a science. Before coming to this point it is necessary to inspect carefully the facts of nature as displayed day by day on the weather maps.

A new interpretation of the weather maps of the United States. On page 223, C. and G. S. Report, Captain Toynbee is quoted as follows: "cyclone winds are formed, as it were, in the hollows of pressure, in which case the ridges traveled with cyclones and formed, as it were, part of them. In many cases the ridges, with

corresponding winds, extended over many more degrees of latitude than the cyclones themselves." Ferrel says of this, "it would be difficult to explain, upon any known principles, the existence of mere longitudinal ridges of high pressure; but that there is such a ridge surrounding every cyclone we have seen is strictly in accordance with well-known mechanical principles as developed in the theory of cyclones." I must admit freely that I am unable to see in the daily weather maps that formation as fundamental which Ferrel and meteorologists generally assume to be the primary state. The usual order or sequence of the phenomena has been regarded as follows, pressure, temperature, precipitation, the cyclonic and anticyclonic system due to unknown causes modifying the normal isothermals and the precipitation. I propose to see in temperature differences, arranged in ridges or waves, the true cause of the observed pressures and the antecedent of the precipitation. It is therefore necessary to account for cold and warm temperature waves passing over the United States.

It will be desirable to refer to easily accessible maps, in order to come within the proper limits of this paper. In Dunwoody's admirable charts giving a summary of the international meteorological observations, 1878 to 1887, the maps of normal pressure and temperature, with prevailing winds, for each month of the year are here employed. A line was traced along the crest of the maximum pressure, and for November its course is as follows; over the Atlantic in latitude 30°, then over north Africa, south Italy, north Black and Caspian Seas, central Siberia, Corea, Pacific Ocean near latitude 30°, central California, Colorado, Kansas, Tennessee, Carolina; there is also an atmospheric shunt, passing from central Siberia near the geographic and the magnetic poles, through British America, and Alberta, to Colorado. Thus Ferrel's polar cyclone is broken up into two permanent cyclones, in the North Atlantic and the North Pacific respectively, by the influence of the continental masses upon the temperature and the dependent densities of the air. This shunt persists from September to June inclusive, in nearly the same position, though oscillating across the polar regions with the season to some extent, but during July and August is reduced to a short spur projecting into British Columbia. Within the two cyclonic ridges the winds circulate anti-clockwise, outside the principle circuit they circulate clockwise, diverging from the central calm of maximum pressure, as Ferrel's theory of general motions demands. This outflow, in conjunction with the right handed deflecting

force due to the rotation of the Earth, deepens the permanent cyclones, heightens the maximum pressures, and lowers the equatorial pressure belt. The passage of the winds past each other in opposite directions tends towards local gyrations, which all drift eastward with the prevailing component in middle latitudes. All this depends simply upon the difference between polar and equatorial temperature, and is fully in accordance with the views of Ferrel and the latest expressions by Dr. Hann.

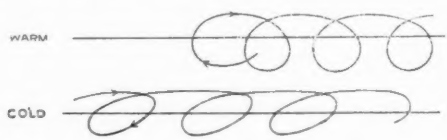
It is, however, necessary to go a step further to pass out of this generalized condition of affairs, into the specific individual case of a given cyclone. In order to fix ideas, I will describe a typical map, Thursday, P. M., November 16, 1893, remarking that the system indicated prevails from day to day, and whenever obscured by excessive action of certain components tends to return to this normal type. On examining the wind directions, it is seen that there are two curved lines extending from northeast to southwest across the United States towards which the winds are primarily directed. The first passes through Minnesota, Nebraska, Kansas, Oklahoma, New Mexico, near Duluth, Sioux City, Dodge City, El Paso; the other through western New York, western Pennsylvania, West Virginia, east Kentucky, east Tennessee, near Atlanta and Mobile. West of curve number one, the region is cooler; between the two, the region is warmer, and east of number two, it is cooler. Thus a warm section lies between two cooler. It will be observed that these division curves are about perpendicular to the crest of the maximum pressure; by following such curves from day to day it will be found that they drift across the United States, maintaining their general distance apart, until signs of disintegration set in. On the average the curve which cuts the maximum crest at Cheyenne is after 24 hours near Dodge City, after 48 hours near Oklahoma City, after 72 hours near Nashville, and after 96 hours just over the Atlantic coast. This represents the average eastward storm drift for October. Along these curves usually lies a region of calms or very variable winds, the existence of these often serving to locate the dividing curves. They are strictly associated with the cyclonic and anticyclonic pressures so well known.

The formation of these low and high pressure areas is the result of the existence of the warm or cold sections of waves lying athwart the maximum crest. Let us assume for a moment that such temperature differences exist, amounting to 20°-30° F. or even more, and trace the course of the winds. From first principles the warm and cold masses will be impelled towards each

other, because of the action of gravitation on media of differing density. They will tend to encounter along or near the ridge of greatest temperature variation. Take the eastward moving cold air first. North of the maximum ridge the surface component from the general motion is eastward, and there is also the northward component, due to the normal temperature gradient, which maintains the ridge over the outflow on the sides, so that the winds flow in a northeasterly curve, even overcoming the right handed deflecting force of rotation; the result is seen over the Dakotas and New England as feeders to the cyclones there. Upon the maximum crest the lateral components are very small, and the air flows S. E. almost squarely up to the dividing curve, as seen in Nebraska, or New Jersey, and much better on the map for the following morning, Nov. 17, 8 A. M., Friday, where the system has moved in more nearly central to the map. South of the maximum crest, the south lateral component is met by the west general component which acts locally in the same direction as the right handed deflecting force of rotation, all three conspiring to produce the observed anti-cyclonic gyration.

Next observe the course of the warm winds flowing towards the northwest to meet the cool air beyond the ridge. There is the northwest temperature gradient, the right-handed deflecting force, and the general eastward surface component of the middle latitude, all of which act to turn the direction rapidly towards the right, as the prevailing arrows plainly show. There is no splitting to the left and the right of the maximum crest, as in the east-flowing cool air, but all goes smoothly from S. E., more and more curving to the north. Along the line of greatest temperature change with cold air to the west and warm air to the right of it, the gyrating cyclone is formed, the couple existing from the system of causes thus described. Likewise, along the next ridge, with cold to the east and warm to the west, and often to the south of the maximum crest, the anti-cyclone is produced. It must be remembered that if the warm and the cold waves are formed, the flow will be both east and west from each successive section, with the view to produce temperature equilibrium. The discontinuous effects of large masses of air is such as to enable these waves to persist many days, possibly enough to go from Alberta, across the United States to Europe. A corollary remark is that the storm track along the north United States seems to be the effect of the general circulation to restore the permanent polar low belt which is interrupted by the continent. Another is that tornadoes and hurricanes are due to precisely the

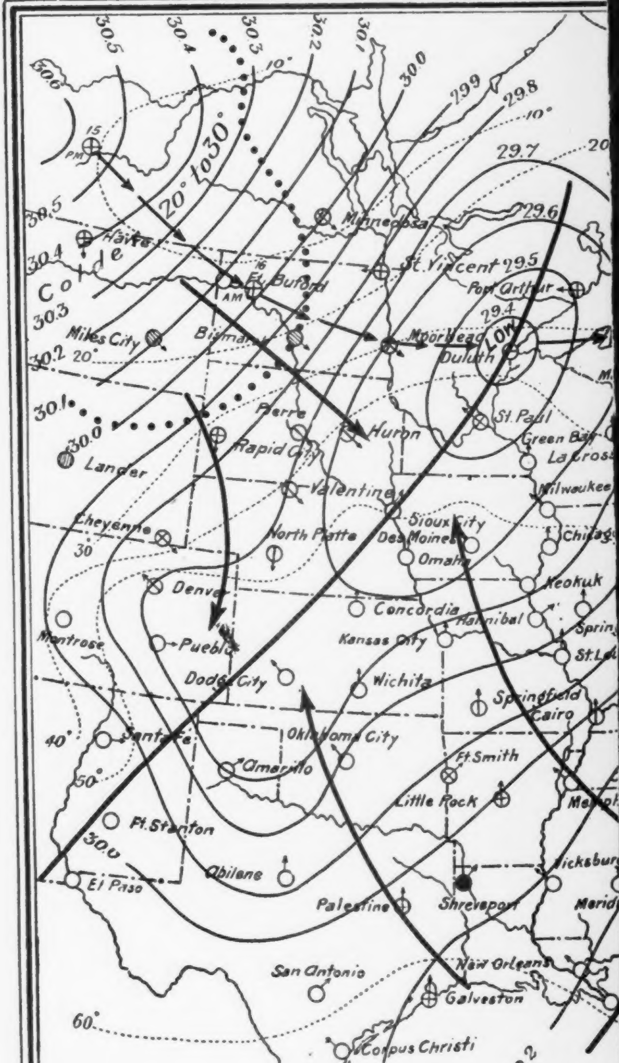
same cause, namely the juxtaposition of masses of air having great temperature differences. The debris of tornadoes shows precisely the same distribution of air as that pointed out above; their tracks being along the course of the dividing line. Hence, the forward gyration, due to the feeding from the south and southeast sides, combined with the general eastward motion, gives them the apparent northeasterly curved path, and locates them on the south side of the cyclonic depressions. It is necessary not to extend this explanation further at this time, in order to arrive at the final step in the argument. This system gives us a surface phenomenon almost exclusively, the cyclonic outflow at a moderate altitude being caught up with the more rapid easterly component; and similarly for the anti-cyclone. The total effect of the upper and the lower circulation is that the warm and



the cold layers are rolled over and over while they advance eastward, the greatest interchange of temperature taking place near the surface of the Earth at the line of contact. Thus the eastward high component may readily deposit warm masses over cold, or cold over warm, as the case may be, according to the kind of air lying to the west of the lower mass in question. So far as I can see all goes on in conformity to Ferrel's principles, provided the warm and cold waves can be produced in the proper sequence, and referred to an efficient cause in nature. This will be the final step to be taken, and the way now proposed is, I believe, novel in the history of meteorology.

Origin of the transverse temperature waves. The method pursued to unmask the features of the United States Temperatures is so efficient in elucidating the entire subject, and forms so rapid a means of verifying the theory of polar radiation, that it is recommended to students of the topic. At the side of the daily weather map is tabulated a synopsis of the meteorological stations, in the fourth column of which occurs the temperature. This column was broken up into sections, and the name of the last station included in a block taken to indicate the district covered by the stations. They were chosen to bring out the wave feature as far as possible, but it is evident that a complete re-arrangement of the order of the stations would greatly facilitate such a purpose.

Novemb



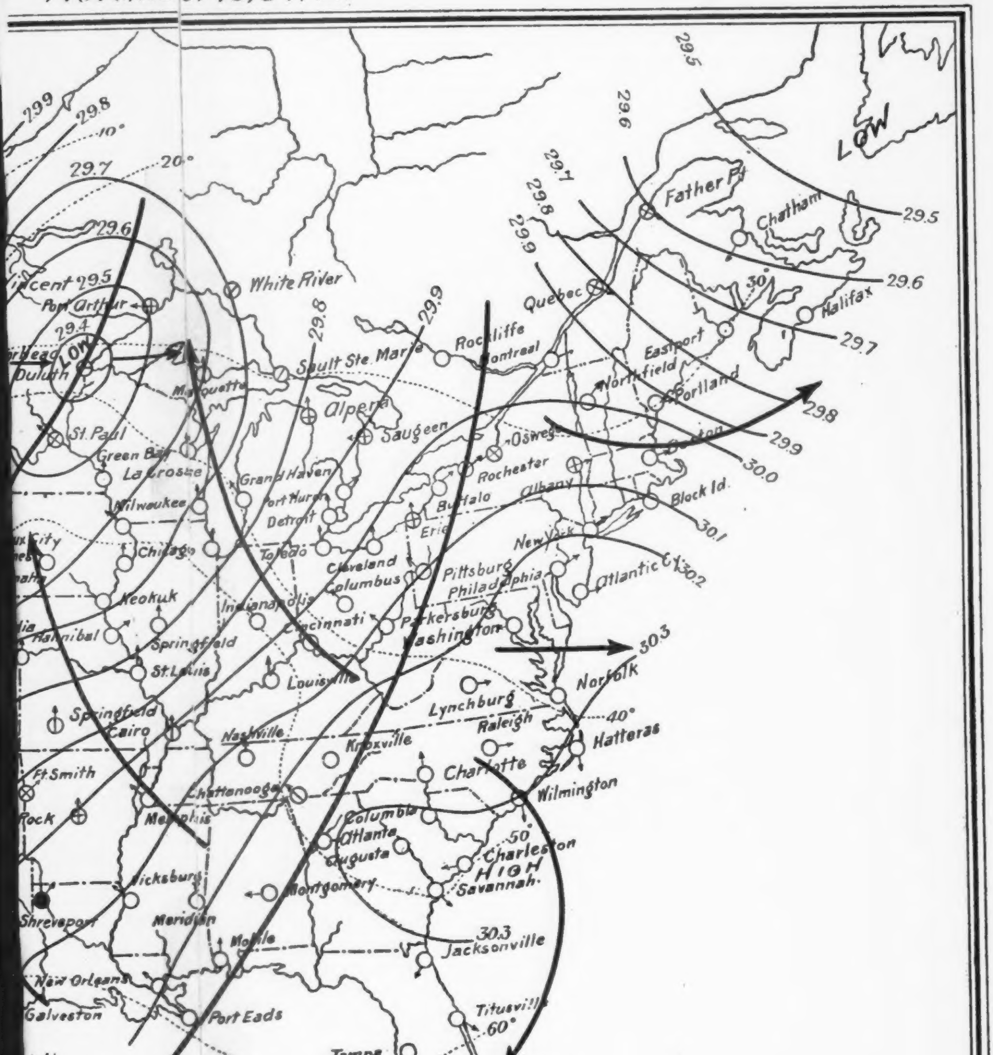
Observations taken at 8 A.M. and 8 P.M. 75th Meridian Ti
Barometer reduced to Sea level and 32° F.
→⊕→ Storm track and location of Storm Centre.
Date above, time (A.M. or P.M.) below

○ Cloudless. ◐ Partly Cloudy. ⊕ Cloudy. ● Rain. ◍ S

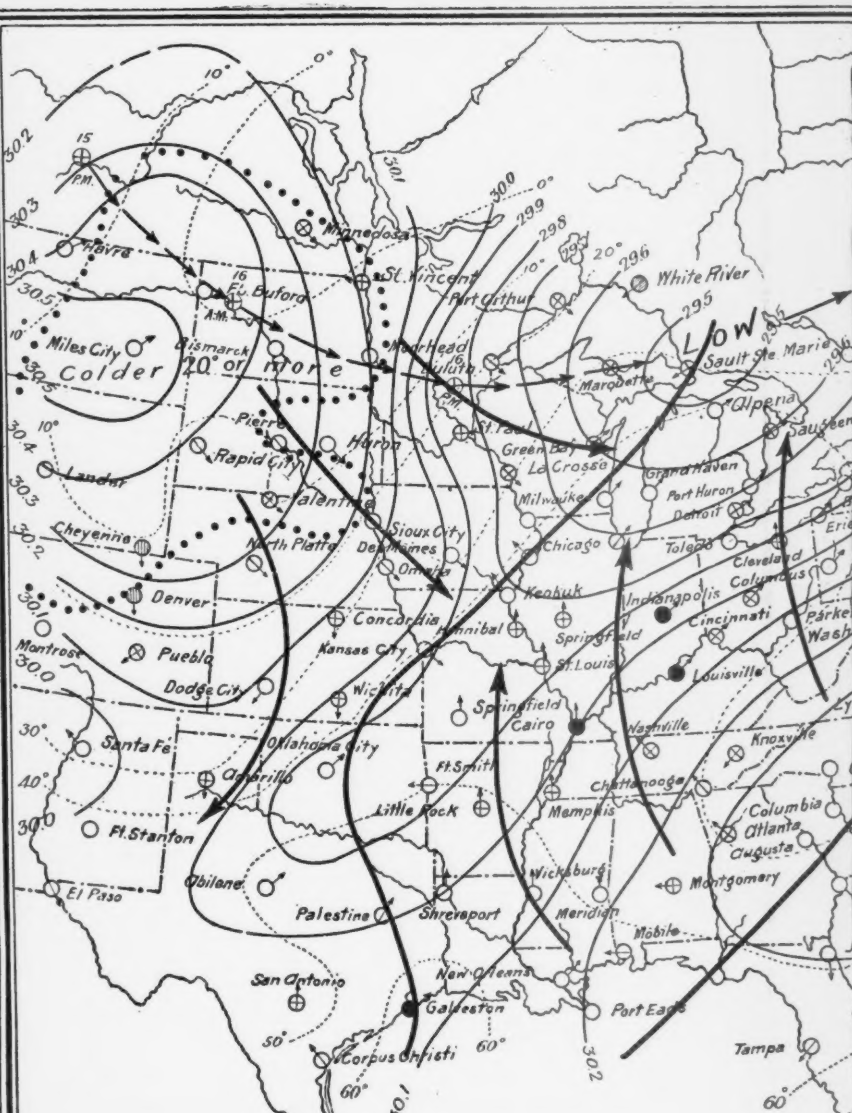
—— Isobars. ------ Isotherms.

Arrows point in the direction the wind is blowing.

November 16, 8 P.M.



November 17, 8 a.m.

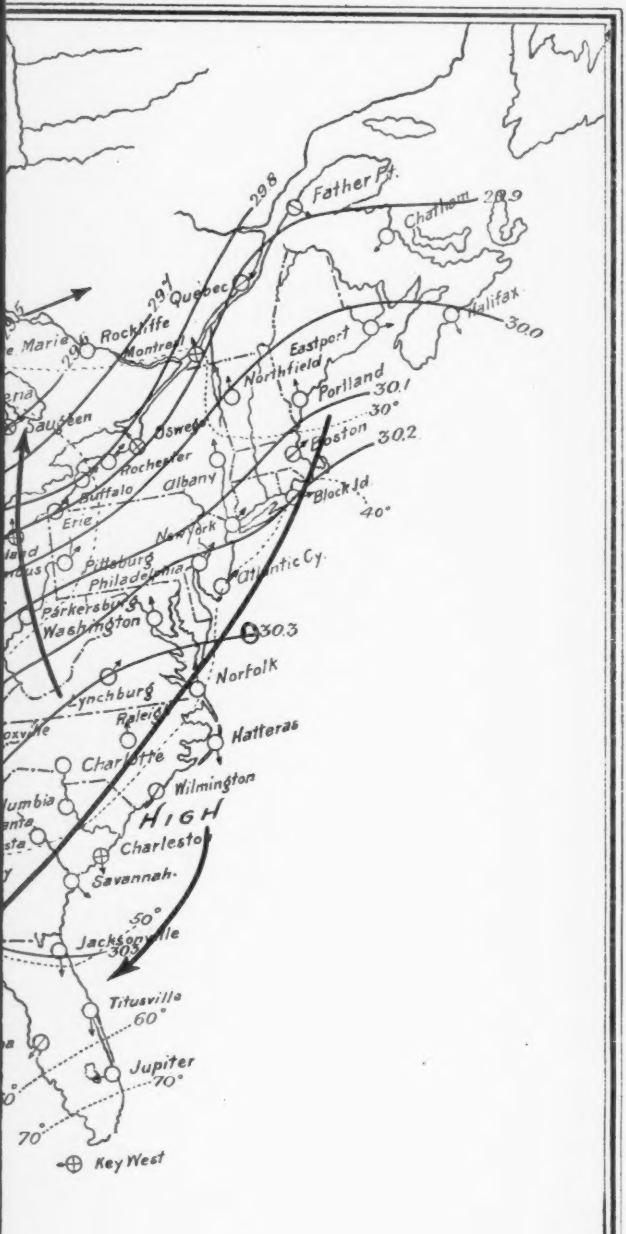


Observations taken at 8 A.M. and 8 P.M. 75th Meridian Time.
Barometer reduced to Sea level and 32° F.
→⊕→ Storm track and location of Storm Centre.
Date above, time (A.M. or P.M.) below.

○ Cloudless. ◑ Partly Cloudy. ⊕ Cloudy. ● Rain. ◍ Snow.
—— Isobars. ------ Isotherms.

Arrows point in the direction the wind is blowing.

7.

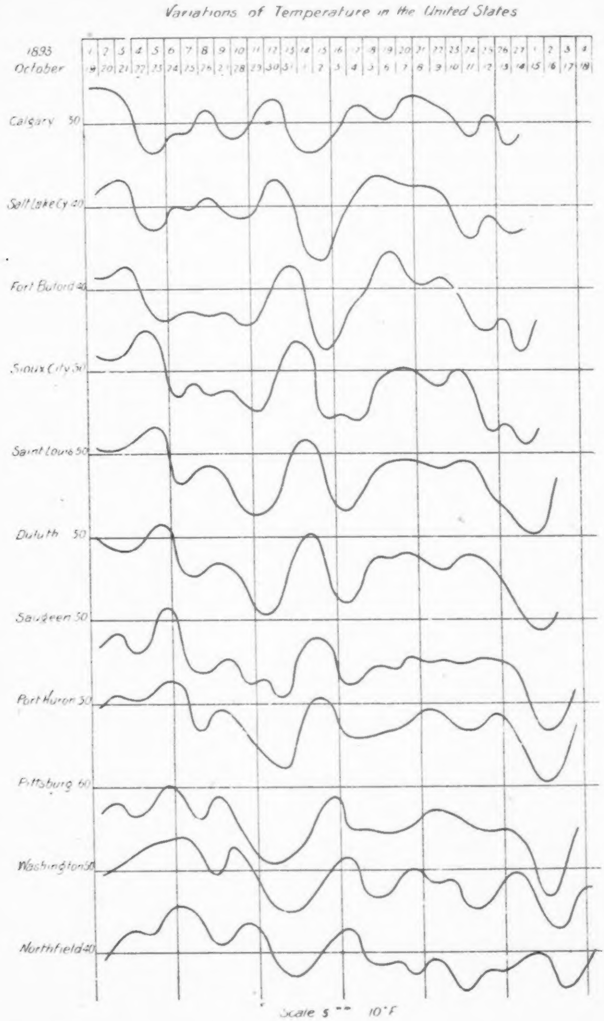


Father Pt.
Chatham
29.9
29.8
Quebec
29.7
Rockliffe
Montreal
Halifax
30.0
Eastport
Northfield
Portland
30.1
30°
Oswego
Rochester
Boston
30.2
Buffalo
Albany
Block Id.
40°
Erie
New York
Pittsburg
Atlantic Cy.
Philadelphia
Parkersburg
Washington
30.3
Norfolk
Lynchburg
Raleigh
Hatteras
Charlotte
Wilmington
HIGH
Charleston
Savannah
50°
Jacksonville
30.3
Titusville
60°
Jupiter
70°
70°
Key West

The group stations are, Northfield, Washington, Key West, New Orleans, Corpus Christi, Pittsburg, Saugeen, Port Huron, Duluth, St. Louis, Sioux City, Fort Buford, Salt Lake City, Tucson, Roseburg, Yuma, Calgary including the Manitoba and Alberta Stations. A long roll of millimeter paper was laid out, with one centimeter per day, and the stations in nearly reversed order beginning west and working east, Y, R, C, S. L. C, F. B, S. C, T, C. C, S. L, D, S, P. H, P, N. O, K. W, W, N, so that a transverse wave will pass along certain groups in succession. The A. M. and P. M. mean temperatures for each group are then plotted, the dots joined, and the broken line allowed to grow. Beginning with summer, where the amplitudes are not wide and the form more or less featureless, as the season advances, marked typical formations appeared which persist, and which were recognized as those of the curve of magnetic intensity of the coronal field, whose derivation has been described in the papers mentioned above. Over the current calendar dates were placed the corresponding numbers of the magnetic ephemeris. The dates equivalent to the first day of the 26.68 day period, are for the present purpose: 1893, Aug. 27, Sept. 22, Oct. 19, Nov. 15, Dec. 11; 1894, Jan. 7, Feb. 3, and so on, adding 26.68 days for the interval. After two or three periods were thus developed it appeared that the magnetic curve, placed strictly according to the ephemeris, matched with the temperature curve at the northwestern districts, but that a lag took place in the districts further east, as the temperature sequences travelled along the highway of the maximum pressure. The lag is as follows, for October,

for Yuma, Roseburg, Calgary, Salt Lake City,	+ 0 days.
Fort Buford, Sioux City, Tucson,	+ 1 day.
Corpus Christi, St. Louis, Duluth,	+ 2 days.
Saugeen, Port Huron, Pittsburg,	+ 3 days.
New Orleans, Key West, Washington, Northfield,	+ 4 days.

The vigor of the temperature curves is greater on the north than on the south side of the axis of high normal pressure, since the action of the equatorial electro-magnetic field tends to diminish the distinctive action of the polar magnetic field. The accompanying diagram of curves, for the north districts, shows the progression of the temperature wave across the United States. The ordinate for the day is the mean 24-hour temperature nearly, being actually the mean of the 8 A. M. and 8 P. M. observations. The scale is 1 m.m. = 2° F., so that the fluctuations include all the temperature variations with which the problem of meteor-



ology has to deal. The progress of the waves is seen and the gradual diminution of the energy as the process of thermal interchange goes on. From the fact that they move about 2,500 miles in four days, with decay to about two thirds their maximum value in the northwest, it seems probable that many of them will cross the Atlantic before extinction on the European continent, as the storm tracks show is often the case.

Temperatures of the United States by Districts.

North.

(From Oct. 19, to Nov. 14, 1893.)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Calgary.	40	40	37	24	21	27	28	34	26	26	33	37	28	20	23
Salt Lake City	44	46	48	34	33	40	39	42	38	36	39	47	42	26	23
Fort Buford	45	47	37	31	32	33	32	33	29	32	42	47	32	21	29
Sioux City	54	57	63	57	42	46	43	44	40	38	48	59	56	36	37
St. Louis	53	57	57	41	44	46	41	33	32	38	52	54	41	33	38
Duluth	46	50	54	40	39	42	41	34	27	32	45	51	40	31	36
Saugeen	43	54	44	34	36	38	31	32	27	37	44	43	30	34	36
Port Huron	53	57	55	42	48	46	40	35	31	40	51	50	40	40	42
Pittsburg	54	60	58	51	57	51	43	37	38	42	50	57	48	48	46
Washington	59	60	55	48	57	52	42	38	38	44	50	54	43	42	46
Northfield	53	54	49	42	46	49	41	33	32	38	44	47	37	36	37
	49.5	52.9	50.6	40.4	41.3	42.7	38.3	35.9	32.5	36.6	45.3	49.6	39.7	33.4	35.7
	+ 9.3	+ 12.7	+ 10.4	+ 0.2	+ 1.1	+ 2.5	− 1.9	− 4.3	− 7.7	− 3.6	+ 5.1	+ 9.4	− 0.5	− 6.8	− 4.5
	+ 19	+ 25	+ 21	0	+ 2	+ 5	− 4	− 9	− 15	− 7	+ 10	+ 19	− 1	− 14	− 9

South.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Yuma	56	57	55	55	58	57	57	57	61	56	57	56	54	52	54
Roseburg	48	50	48	44	43	40	45	49	49	49	48	50	40	34	38
Tucson	53	55	54	51	46	50	49	46	44	46	51	53	50	36	40
Corpus Christi	63	64	67	67	68	66	55	57	55	56	61	66	67	63	58
New Orleans	62	64	65	66	63	58	53	50	51	55	62	65	64	58	54
Key West	66	66	66	66	69	63	55	50	53	55	60	63	65	63	59
	58	59	59	60	58	56	52	51	52	53	57	59	57	51	51
	+ 4	+ 5	+ 5	+ 6	+ 4	+ 2	− 2	− 3	− 2	− 1	+ 3	+ 5	+ 3	− 3	− 3
	+ 8	+ 10	+ 10	+ 12	+ 8	+ 4	− 4	− 6	− 4	− 2	+ 6	+ 10	+ 6	− 6	− 6

North.

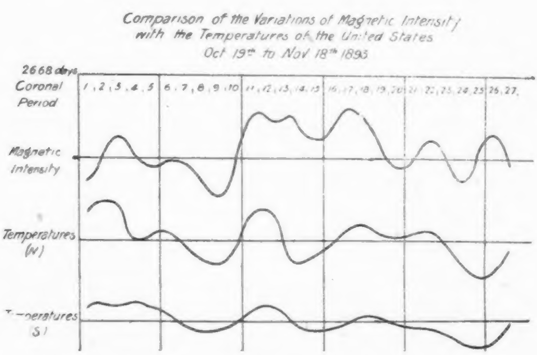
	16	17	18	19	20	21	22	23	24	25	26	27		
Calgary	28	35	33	31	37	36	34	31	25	32	23	26		
Salt Lake City	35	44	49	48	46	46	43	35	29	37	32	33		
Fort Buford	37	42	51	44	41	43	39	29	28	30	21	30		—1 day
Sioux City	35	47	50	50	48	45	50	42	32	34	28	33		—1 day
St. Louis	45	48	48	47	45	48	45	37	33	27	26	42		—2 days
Duluth	44	44	45	42	40	44	44	40	34	26	22	28		—2 days
Saugeen	35	39	37	38	37	39	38	37	26	17	19	29		—3 days
Port Huron	43	45	48	45	42	43	47	41	34	26	30	44		—3 days
Pittsburg	47	49	53	52	50	47	47	46	40	26	33	48		—3 days
Washington	50	46	47	39	39	43	49	46	36	32	43	44		—4 days
Northfield	33	38	34	27	33	33	37	40	38	29	32	40		—4 days
	39.3	43.4	45.0	42.1	41.7	42.5	43.0	38.5	32.3	28.7	28.1	36.1	40.2	
	− 0.9	+ 3.2	+ 4.8	+ 1.9	+ 1.5	+ 2.3	+ 2.8	− 1.7	− 7.9	− 11.5	− 12.1	− 4.1		
	− 2	+ 6	+ 10	+ 4	+ 3	+ 5	+ 6	− 3	− 16	− 23	− 24	− 8		

South.

	16	17	18	19	20	21	22	23	24	25	26	27		
Yuma	56	55	54	52	55	54	55	57	58	57	56	59		
Roseburg	46	50	53	49	53	49	45	46	45	46	43	42		
Tueson	42	44	47	47	40	43	40	32	34	36	36	40		—1 day
Corpus Christi	55	55	59	59	57	56	65	58	49	45	43	56		—2 days
New Orleans	55	56	56	58	56	56	56	50	43	44	57	62		—4 days
Key West	57	57	59	57	55	56	58	58	51	45	52	57		—4 days
	52	53	55	54	53	52	52	50	47	46	48	53	54	
	− 2	− 1	+ 1	0	− 1	− 2	− 2	− 4	− 7	− 8	− 6	− 1		
	− 4	− 2	+ 2	0	− 2	− 4	− 4	− 8	− 14	− 16	− 12	− 2		

Now by shifting the daily temperatures backward, by 1, 2, 3, or 4 days, so that the same phases of wave temperature fall into their own columns, the mean effect can be found for each wave, and thus the masking effect of successive waves at individual stations can be eliminated. It thus is clear that the application of the magnetic ephemeris and the lag interval to meteorological data, is the proper method to unmask the superposed forces that have hitherto concealed the true functional relations. The tracing of the lag of the curve across the Atlantic has not been undertaken; indeed it will be necessary to study the whole hemisphere in this graphical way, before the subject can be fully mapped out in its physical topography. The table of temperatures gives the actual readings, the lower districts all being shifted to the left as indicated. The stations are separated into north and south groups, divided by the belt of maximum pressure, as nearly as our data permit.

The temperature residuals are taken out from the mean of the 27 days, and multiplied by the factor 2, in case of plotting on half-millimeter paper. As they stand in the reproduction in ASTRONOMY AND ASTRO-PHYSICS, 5 m.m. = 10° F.



Magnetic scale 5 mm. = 0.000020 C. G. S.
Temperature scale 5 mm. = 10° F.

Finally the original magnetic curve, which represents the variable intensity of the coronal or polar field surrounding the Sun, as derived from the magnetic and meteorological data of Europe, the Atlantic and United States, for the years 1878—1892, as far as possible (see *Amer. Meteorol. Journ.*, Sept., 1893), is here reproduced. Its base line gives the mean intensity of this solar field at the distance of the Earth, equal to 0.000115 C.G.S.

units, and the scale of variation is 5 m.m. = 0.000020 C.G.S., so that the range of fluctuation is about 0.000050 C.G.S., the amplitude proper being about one-fourth of the total mean intensity. Underneath this is placed the two curves of temperature, one for the districts north, and the other for those south of the summit of the pressure crest. They represent the effect of one rotation of the Sun on its axis, carrying this variable magnetic field past the Earth, upon the temperatures of the United States. The harmony of the curves speaks for itself significantly. The residuals or relative numbers obtained from the use of about 200 revolutions of the Sun, of course eliminated the masking effects of superposition, while the residuals were much cut down in size. No evidence could be more striking than this, that the direct action of the solar polar magnetic field is responsible for the temperature waves which build up the cyclonic and anticyclonic system of pressures and winds in the United States.

The conclusion now follows simply from this set of premises. The solar magnetic field represents a type of radiant energy, probably circular or spiral rotation of the ether, which surrounds the Sun on all sides, but of variable strength in certain solar longitudes. In other words the earth passes through a series of hotter and colder regions as the Sun turns on its axis. One day is the equivalent of about 10,000,000 miles. Since the form of energy is magnetic, which of course means a special form of ether motion, this energy approaching the earth, itself a magnetic body capable of conducting the lines of force better in some directions than in others, is concentrated or focused in the magnetic ovals surrounding the magnetic and geographical poles. The form of the regions of concentration came out fully in my study of the equatorial radiant field. Thus the atmosphere around the polar regions is intermittently heated or cooled according as more or less of this polar energy falls upon it, the temperature being a direct function of the radiant energy.

It will be remembered that the atmospheric shunt crossing the polar regions passes directly through this area, which is thus subject to varying absorption of radiant energy and is therefore alternately heated and cooled, strictly in response to the solar field. This air is transported by the general motions of the atmosphere around the permanent cyclones, on the east side if we look from the United States, downwards into the well known storm belt, where the transverse temperature waves pursue the path already described; on the west side of the polar shunt the circulation is towards the East Asian coast and Pacific ocean, where a similar

series of temperature waves probably exists. The larger masses will, without doubt, be carried into the United States owing to the fact that the magnetic pole on the American continent disposes the radiation more on that side of the polar regions than on the other.

The case may rest here for the present while the consequent mathematical developments are being worked out. Only one period has been described, but a dozen lay before me on the sheet; and the constant recurrence of this solar curve during 15 years in the pressures of the northern hemisphere, makes it safe to accept this solar polar field as the true origin of the energy of Highs and Lows, so long sought in practical meteorology. Instead of being an obscure term in meteorology, magnetic energy comes to the front as the primary factor in all the weather conditions, certainly of the United States. The strict response of temperatures, upon which all else depends, to the peculiar variations of the magnetic field, on individual dates, makes it not only possible to forecast the weather conditions specifically, by means of the ephemeris and the lag, but it is the key to the reading of the weather maps. The intensities of the successive waves can be read off, the form of a wave can be located to the width of a single state, and from the waves the wind system and the accompanying cyclones and anticyclones can be laid down very closely. Of course the ragged edge of temperatures along the wave fronts, it will be hard to determine by any method now known, but this penumbra is limited and wholly unimportant. It will be necessary to study the lag sequence throughout a number of years to obtain its characteristics and, at the same time it will be important to learn the effect of the change of the Sun in declination. The waves flat out in summer, chiefly because the polar shunt disappears, and in the winter the circulation may become too violent to leave the waves in their integrity. The reversal of the whole system of temperatures is found to take place in December, when the Earth passes from the positive to the negative hemisphere of the Sun, and the curve must be inverted to follow the temperature variations. This can all be determined by some further experience. It is also to be observed that the areas of precipitation are closely associated with the lines marking the contact of the warm and cold wave, and that this is the product of condensation by cooling of mixtures, and not by cooling through dynamic expansion. Many of the features of physical meteorolgy will thus find in this system their true criticism. I have not yet drawn out the function of the magnetic field and tempera-

ture, because the former was taken from European data, which is too far removed from the concentration area. The polar stations of 1882-1883 will probably supply the requisite material.

It should be noted that there may be found in this polar radiation the true cause of the great ranges of temperature in the polar regions, known in the Glacial Epochs. If the Sun through long periods of time changes the quantity of its magnetic output, and it will if it is a variable star, the effect would be to cool the poles of the earth, if it were less, and to heat them if it were more, a series of changes that Geology thinks has taken place. It is only necessary to add a long geologic period to the 11 year and the 27 day periods of the Sun. Also it may be noticed that this perodic temperature fluctuation which is followed by variations in the wind circulation, and to some extent in that of the water, may have something to do with the variations of terrestial latitudes observed to have been taking place. Across the atmospheric shunt more or less air will be drawn according as the velocity in the general cyclonic circuits is increased or diminished and this will be accompanied by the formation or melting of snow and ice, which may affect the movement of inertia of the Earth to some extent. It is seen, as stated heretofore in the preceding paper, that the intensity of the atmospheric circulation of the hemisphere increases or diminishes, as shown by the number of Highs and Lows on the synoptic charts, with the variations of the solar period. Chandler obtains 427 days as the period of the variation of latitude. Now $16 \times 26.68 = 426.88$, that is, sixteen solar periods are equal to the probable latitude period. This may be a coincidence and is not insisted upon, but it can readily be supposed that the great range of temperature on the polar regions, may have an important effect on the accumulation of ice and snow, in some short period, as well as in a very long glacial period. Correlative factors are at work on the Southern hemisphere, but I have no experience with them.

It goes now without saying that the development of magnetic observations in connection with meteorological stations will be necessary to derive a full advantage of this theory. Much can be done with the mean magnetic curve in forecasting the weather at long range or short range; but I advocate the extension of the European system of magnetic observation, or their practical equivalent, to the American continent. I thought I observed that individual observatories were sensitive to variations of the magnetic field a thousand miles away but that can be determined by experience. The subjects of magnetic storms, auroral

displays, earth currents, are evidently subordinate manifestations of this solar polar magnetic field.

There is now certainly great encouragement to the meteorologist to review the rich material at his disposal, with the object of making a scientific analysis of the subject, which has not yet passed beyond the empirical stage. There will always be a nebulous fringe of uncertainty on weather forecasts, owing to the fact that we deal with a rapidly moving fluid, but I believe that a great improvement in weather predictions, both at long and short range, is in sight if not at the doors. There are now several steps to be taken to improve meteorology.

1. Substitute the solar magnetic ephemeris for the common calendar.
2. Employ the mean magnetic curve as a guide to interpret all variations.
3. Determine at each station the lag of the wave from the polar region.
4. Determine the law of the magnetic reversals of temperature.
5. Form the equations of local motions, and work out the physical constants.

A NEW STAR IN NORMA.*

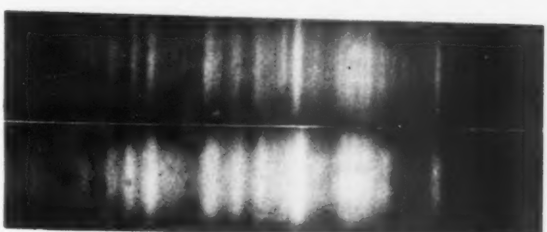
EDWARD C. PICKERING.

A new star appeared in the constellation Norma during last summer. It was discovered by Mrs. M. Fleming on October 26, when examining a photograph of the spectra of the stars in its vicinity. The photograph was taken on July 10, 1893, at the Arequipa station of this Observatory by Professor Solon I. Bailey. The spectrum appears to be identical with that of the new star which appeared in Auriga in December, 1891. Comparing the spectra of the two stars taken with nearly the same dispersion, about a dozen lines are visible in each and are identical in wave-length. The hydrogen line F, which is bright in both stars, is more intense in the star in Norma than in that in Auriga. It is, in fact, more intense in the former star than any other line in the spectrum, while the G line is generally the strongest in Nova Aurigæ. A photograph taken June 21, 1893, showed the spectra of stars of the 10th magnitude, but no trace of the new

* Communicated by the author.

PLATE IV.

Nova Aurigæ, February 6, 1892.



β α K H h G F

Nova Normæ, July 10, 1893.

Astronomy and Astro-Physics, January, 1894.

star was visible upon this plate, although it covered the same region and was in other respects like that taken on July 10. Photographic charts of the same region taken on June 6, June 10, July 21, 1889; May 16, May 16, June 10, June 23, June 23, 1891; May 7, and May 27, 1893, show no image of this object although stars of the fourteenth magnitude are visible upon some of them. We may therefore conclude that the star appeared within ten days of the first of July and that previously it was either invisible, or extremely faint. The position was found by comparing the ends of the hydrogen lines, G and *h*, which are bright in this star, with the corresponding dark lines in the adjacent stars A.G.C. 20940 and 21006. They give the approximate mean position for 1900 in R. A. $15^h\ 22^m\ 12^s$; Dec. $-\ 50°\ 13'.8$. A more accurate position can be found if photographic charts can be taken showing this star. Professor Bailey has been notified of this discovery and if the star is still bright enough he will doubtless obtain photographs showing its position and spectrum.

The similarity of the spectra of these two new stars is interesting, first, since it has proved a means of discovering one of these objects, and secondly, because if confirmed by other new stars it will indicate that they belong to a distinct class resembling each other in composition or physical condition. The star was approximately of the seventh magnitude photographically on July 10 since it was about equal to A.G.C. 20910 magnitude 6.9 whose spectrum is of the second type. The nearest catalogue stars are A.G.C. 20940, magn. 8, which has a spectrum of the first type, and A.G.C. 20926, magn. 8¾, which has a spectrum of the second type. The new star lies nearly midway between these two.

CAMBRIDGE, Mass., U. S. A.
November 9, 1893.

MR. LANGLEY'S RECENT PROGRESS IN BOLOMETER WORK AT THE SMITHSONIAN ASTRO-PHYSICAL OBSERVATORY.*

All the best work in this direction has hitherto been done by micrometric measurements of the position in which the bolometer indicates heat or cold, a method which in this application, is so slow, that between two and three years of assiduous labor were

* Abstract of a communication made to the Philosophical Society of Washington, May 27, 1893.

required formerly, to make a curve showing only the leading absorption lines of the infra-red spectrum.

The first and most important feature of the new method consists in an arrangement by which the means of visual observations, or micrometric measures of the galvanometer deflections, is done away with, and the record of the movements of the needle as the bolometer thread travels through the spectrum, is made photographically, the result being a curve showing the variations in heat at every point of the invisible spectrum corresponding to the (invisible) absorption lines; and this process has been found so incomparably more rapid than the former, that more lines can be located by it in single day than by the old process in a year's assiduous work.

The new process implies the use of a more delicate bolometer and galvanometer than the old one, and this has been attained to such a degree that the delicacy of the new apparatus is about a hundred-fold greater than the already delicate means used by Mr. Langley, in the researches previously published.

To secure the desired accuracy, it has also been necessary to employ more massive as well as more delicate apparatus, and to pay great attention to instrumental details.

The new, and extremely large and accurate spectro-bolometer, which carries the prism or grating, is provided with azimuth circle, reading to 5 seconds of arc, which can by application of special clock-work to its tangent screw, be given a slow and perfectly uniform motion of rotation, such that the successive portions of the spectrum fall in uniform sequence upon the bolometer thread. The utmost care has been taken to mount all the portions of the apparatus and especially the delicate galvanometer, so that accidental disturbances are, as far as possible, eliminated.

Owing to the accurate clock-work which has just been described as moving the spectrum past the bolometer thread, we can be sure that the movement of the galvanometer, (which records the contact of this thread with any invisible line, and the cold produced thereby) corresponds at any given second of time, to the passage before the thread of some definite portion of the spectrum, which though wholly invisible, can be identified at once, and its wave-length instantly determined by the corresponding reading of the circle, taken in connection with the well determined constants of the great rock salt prism, by which the invisible spectrum is formed. To preserve an automatic record, of this correspondence between the time and amount of the galvanometer deflection, and the coincident circle reading, a sheet of sensitized pa-

per or glass is made to move by the same clock-work vertically before the galvanometer, and a beam of light reflected from its minute mirror, (which is but 2 mm. in diameter,) falls upon the moving sensitive surface, thus recording automatically the time and extent of the deflection. In other words, owing to the synchronism of the movements of the circle, the galvanometer, and the recording films, the abscissa and ordinate of any part of the photographic curve correctly represent the position and intensity of the absorption lines of the invisible spectrum. Such curves were exhibited by Mr. Langley, showing every detail of the thermal variations from the upper end of the visible spectrum down to a wave-length of about 7 microns, that is, through a space nearly 15 times as great as the spectrum known to Sir Isaac Newton.

Each of these curves was obtained automatically in less than a day's time, but in spite of this, each presents a multitude of details such as would have escaped the most laborious personal observations, and by preserving for future study, many such which even an experienced investigator might have overlooked, or if seen, have omitted as being probably due to accidental or unimportant sources of disturbance, these curves have wonderfully increased the power and range of our investigations. By securing so complete a result in so short a time, Mr. Langley has rendered it possible and easy for us to investigate the absorption lines due to the earth's atmosphere; and that too, in all parts of the spectrum, since we have only to compare among themselves, the bolographs made during the morning, midday and evening hours respectively. It may therefore be said that both the chemical constitution and the thermal absorbing power of the atmosphere as a whole, are now brought within the range of scientific study; and this culmination of Langley's work must be hailed as being as important to meteorology as it is to the solar physics; for since the greater proportion of all the new lines mapped, are probably due to absorption in our own atmosphere, and represent. with hitherto unheard of precision the localities and extent of that absorption as exhibited in the invisible spectrum, the effect is nearly the same as if he had discovered some chemical sensitive to the whole ultra-red portion of the spectrum, in the way that silver salts are to the vibrations of the violet end.

Finally through some automatic process which he did not explain, but which would seem to be a simple application of the Woodbury, or gelatine process in photogravure, this bolograph record of the infra-red spectrum is converted into a linear spec-

trum, showing the invisible absorption groups as the lines and bands, so that the final presentation of the invisible heat spectrum resembles, on a smaller scale, the magnificent photographs of the visible solar spectrum that have recently been published by Rowland, and from these, any incidental variations if such exist, are eliminated by a process resembling that of composite photography, but again entirely automatic, and independently of the personality of the observer; the final sheets are thus being prepared for publication,—a publication, however, which is expected to involve another year of labor before entire completion.

Mr. Langley mentioned the obligations of the Observatory to the late Dr. Kidder, and to the generosity of Mr. Alexander Graham Bell, and alluded to the assistance he had received from Messers. Hutchins and Hallock, and especially from Mr. F. L. O. Wadsworth.

THE OBJECT-GLASS GRATING.*

L. E. JEWELL.

During the last two years, experiments in another line of study, have led me to the consideration of methods by which a photographic grating adapted to large telescopes might be constructed. The object sought was not a substitute for either plane or concave gratings ruled upon speculum metal, but a satisfactory grating for obtaining the spectra of stars, where only a fair amount of dispersion is desired, but intensity of light is of the utmost importance. At present the object-glass prism is used, but it is subject to some serious defects and the dispersion obtained is small. A photographic object-glass grating would answer the purpose admirably if certain difficulties could be overcome. Without considering at length these difficulties let us see how the desired results may best be secured.

The plan now proposed will, I believe, be found to be the most satisfactory. It is to photograph a series of images of a long narrow slit, as perfect as one can be made. This can best be done by having the slit and photographic lens fixed, and the photographic plate movable, by placing the latter upon the carriage of a dividing engine. The carriage is moved along by means of the revolution of a perfect screw, which is revolved through a certain angle, and then the exposure made. This process is re-

* Communicated by the author.

peated until the desired number of exposures have been made, when the plate is developed and fixed, the result being a photographic grating.

Such being the general plan of operations, we will now consider the details. The first thing necessary is as perfect a slit as it is possible to secure. It must have no irregularities, and should be a perfectly straight line. I believe the best results will be secured by photographing a wire stretched against a uniformly bright background. It is absolutely necessary to have a photographic plate that is practically structureless, so as to avoid halation, and to so develop it that the image of the wire shall be perfectly clear, while the background is as dark as it is possible to make it. Probably some form of collodion emulsion would give the best result. Whatever kind of film is used, it must be perfectly clear and uniform, or it will be useless for delicate work. In addition the film must be hard and firm, and possibly be capable of taking a polish without destruction of the image. This might be secured by a second coating of very hard collodion which could be polished, when the grating is to be placed in front of an object-glass. It may be found best even with a structureless plate to somewhat underexpose and then intensify it after development. It will also be found best to have the photographic plate as long as a perfectly prepared plate can be obtained, and conveniently handled; for we can then use a comparatively large wire, and thus avoid diffraction effects. It will not be necessary either to have the image of the slit so very narrow. Consequently we can make a more perfect slit; and by placing it farther from the photographic lens obtain the same width of lines in one grating that we would obtain were we to use a narrow slit closer in the lens; and the lines would also be sharper and clearer.

This matter of obtaining a perfect slit is of the utmost importance; and time occupied in making a satisfactory one would be well spent. The photographic lens should be as perfect and well suited for the purpose, as it is possible to secure, and should preferably be of long focus, and a flat field, with the utmost possible sharpness of image.

For properly spacing the distances between the lines or images of the slit, a ruling engine of similar construction to those of Professor Rowland, but larger and with some changes made necessary by the carriage holding a large photographic plate, and the automatic regulation of exposures as well as the spacing, would answer admirably.

The photographic plate which is to be used for making the grating should be of the same character as the plate used for making the slit, and should be as large as can be perfectly made, and satisfactorily handled.

The utmost efforts should be made to obtain a perfect grating of as large a size as possible, for having secured a perfect original, an indefinite number of copies can be made either of the same size or smaller.

Having decided upon the number of lines we desire the original grating to have, the width of the lines compared with the width of the spaces can be regulated by adjusting the distance of the slit, and to some extent by regulating the time of exposure. It is best to regulate the width of the lines by adjusting the distance of the slit from the lens and to make the time of exposure just what is necessary to obtain the sharpest and blackest line. In this way we can secure that width of line in relation to width of spacing, which will give the purest and most brilliant spectrum.

Having secured a satisfactory grating of as large a size as can be conveniently made, a large number of copies can be made, and if the lines and spaces are of equal width, it will not be necessary to make a negative of the original grating and then positive copies from that, for copies made directly from the original will answer perfectly, as there would be no material difference between a negative and a positive of the original grating. Also a large grating can be used for making smaller ones with the same number of lines per centimetre as the original, or a larger number. However it would probably be found desirable to make several large original gratings having different numbers of lines per centimetre, for having secured a few original gratings of this kind, they can be used for copying almost indefinitely if well taken care of, and the copies should be equal to the originals if we use as perfect a copying lens as can be secured, and have the original grating set up with a uniformly bright background behind it. We should make the copies with the same kind of photographic plates as that used for the originals, and more pains should be taken to secure optically perfect plates of glass upon which the copies are to be made, than is necessary with the original gratings.

I believe that gratings thus made will answer best for the purpose for which they were originally designed, *viz.*, for placing in front of the object-glass of a telescope, to be used in the same way as an object-glass prism. One considerable advantage of an object-glass grating over an object-glass prism would be in meas-

uring the displacements of lines caused by the motion of the luminous body in the line of sight. The grating would give a spectrum on each side of the central image of a star; consequently, displacements could be readily measured and could probably be determined with more certainty than with a slit and prism, while the object-glass prism gives us no point of reference.

However there are many other uses to which these gratings may be put. In the case of a ruled grating the grooves may be of almost any shape, they may be of an unsymmetrical shape, and instead of single grooves they may be multiple grooves, or notches, or adjacent grooves may overlap, and as a consequence of these irregularities, gratings so made may be very irregular in the distribution of light in their spectra. In fact nearly all gratings are likely to have some eccentricity of this kind, so that in the study of the distribution of light or heat in the spectrum, a ruled grating is liable to give erroneous results. This is not likely to be the case with a photographic grating made in the manner described, for the lines will be single and symmetrical and the width of the lines compared with the width of the spaces can be so regulated as to give the most brilliant spectrum without any irregularity in the distribution of the light. As to how great a dispersion may be obtained along with lines sufficiently perfect to produce a pure spectrum, that will have to be determined by experiments, as it depends upon the degree of perfection of the photographic plates used. However, the dispersion will undoubtedly be sufficient for the purpose for which this form of grating was originally designed. Whether sufficiently narrow and at the same time perfectly sharp lines with clear spaces can be produced for use in gratings of greater dispersion, depends upon the capabilities of photographic methods and this is yet to be determined.

The photographic gratings described can not well be used except when we transmit the light through them and in connection with a lens or mirror, and in case a slit is used, an additional lens is necessary. But may not a copy of the original grating be photographed upon a metallic mirror having any desired curvature? It can unquestionably be done by making the mirror in question a daguerreotype plate. As to whether or not such a grating would give a sufficiently brilliant spectrum, can best be determined by experiment. It may be objected that a daguerreotype grating will tarnish and fade. This is perfectly true and the only remedy is to make another one which can readily be done at small expense and with but little trouble. It may also be that other photographic processes will give better results. If so, a

photographic grating upon any surface may be made as readily as upon a plane surface, and this will be an advantage over a ruled grating in one respect at least; for in ruling a concave grating the grooves are not likely to be of the same form in the middle and at the sides. Another advantage would arise from the large size of the original grating, the spacing of the lines being from this cause necessarily more regular, and this advantage would be retained when the size of the grating is reduced in copying. It is very doubtful, however, if at present a photographic grating could be made that would successfully take the place of a ruled grating where large dispersion is desired.

MARIETTA, Ohio.

ON THE NEW STAR IN AURIGA.*

H. C. VOGEL.

II.

Before I proceed in the next section to a consideration of the most important hypotheses which have been advanced to account for that wonderful celestial phenomenon, the blazing forth of a new star, I have still to mention some results, particularly those of the photometric observations, which were in this case specially characteristic of the apparition.

At the time of its discovery by Dr. Anderson of Edinburg, on Jan. 24, 1892, the star was between the 5th and 6th magnitudes. From the numerous photometric observations which were made at different places after the discovery became known, it appears that between Feb. 1 and March 6 the Nova fluctuated in brightness between the 4th and 6th magnitudes. A first maximum fell between the 3d and 6th of February, a second occurred, on the 18th of February, and a third on the 2d of March. The first minimum was on Feb. 16th, a second on Feb. 23d. It may be assumed that more frequent but smaller fluctuations occurred, particularly in the interval between the 3rd and the 9th of February. From the 6th of March to the 1st of April the light-curve falls off very abruptly to the 13th magnitude.

According to the observations made at the Lick Observatory, the light-curve shows a still steeper descent in the early days of

* Continued from the December number. The greater part of section II is omitted, as it consists of an abstract of observations which have already been printed in full in ASTRONOMY AND ASTRO-PHYSICS. Some remarks on the omitted portions will be found in AstroPhysical Notes.—*Tr.*

April, but falls off somewhat less rapidly from April 8th to April 26th, (the date of the last observation with the 36-inch refractor), when the brightness of the star had sunk to the 16th magnitude.

It is a matter of great interest that several photographs of that part of the heavens in which the Nova appeared were made at the Harvard College Observatory in December, 1891. The Nova does not appear on a plate taken Dec. 1, but it does appear on the next one, taken on Dec. 10th, as a star of the 5.4 magnitude. From the 10th of December, 1891, to the 20th of January, 1892, twelve photographs were taken, which show that the Nova reached a maximum of brightness (4.5 mag.) on the 20th of December.

It is to be regarded as a very fortunate circumstance that a plate of the same part of the sky, taken by Dr. Wolf in Heidelberg, falls precisely in the great gap in the Cambridge photographs. It was taken on Dec. 8, 1891, and does not contain the Nova, which must therefore have been fainter than the 9th magnitude. According to this, the outburst of the star must have taken place very suddenly.

Many determinations of the brightness of the Nova have been made photographically, and they are so far interesting that they show a more rapid diminution of light than the visual observations. In this they are in harmony with the spectroscopic observations, according to which the light fell off very rapidly in the violet, as would be expected in the spectrum of a cooling body.

On Aug. 17, 1893, the Nova was rediscovered at the Lick Observatory as a star of the 10.5 magnitude.* The star had therefore diminished in brightness in October and November, but in December of last year and the beginning of this it had again reached the 10th magnitude.

At the occasion of the Nova's rediscovery, several of the Lick Observatory astronomers observed that the appearance of the star was different from that of other stars of the same magnitude, but the Moon was near and the observations were difficult on account of the brightness of the background. With the 36-inch refractor on August 19, Barnard† found the Nova to be a nebula 3″ in diameter with a 10th magnitude star in its center. This appearance has not materially changed throughout the further observations that have been made. This brightness of the nucleus, as well as that of the nebulous envelope, has under-

* Publications of the Astronomical Society of the Pacific, Vol. IV, p. 243.
† Pub. A. S. P., p. 244; A. N., 3143.

gone fluctuations, but the diameter has remained constant. In all, sixteen observations by Barnard, from Aug. 19 to Dec. 5, have been published.

At the Pulkowa Observatory, Ring and some other astronomers observed a similar aspect of the Nova. The Nova appeared as a minute star, surrounded by a nebulous aureole.*

The photographs taken by Roberts† with his 20-inch reflector, on Oct. 3, 1892 (exposure 110 min.), and on Dec. 25, 1892 (exposure 20 min.), do not show a nebulous envelope, which shows that the nebulosity could not have been more than 21″ in diameter, this being the diameter of the star image on his first plate.

Since it cannot be assumed that so excellent an observer as Barnard could have been deceived, his observations are well worth a more careful consideration; it would surely be of the highest interest to determine with certainty whether the Nova suddenly changed into a nebula, or whether its stellar character was retained in its second appearance.

I think that I can now give a very simple explanation of the peculiar appearance of the star as seen with the great refractors.

In the refractor of the Lick Observatory, the distances between the focal planes for rays of different wave-lengths are very considerable; for example, the difference of focus for F and $H\gamma$ is, according to Campbell, 37 mm., and for $H\gamma$ and $H\delta$, 34 mm. Although the magnitude of these differences is somewhat striking, they are relatively no greater than in other visually achromatized telescopes, as may be seen by referring to my investigation of the circles of chromatic aberration in different telescope objectives.‡ The achromatism of the 36-inch objective seems to differ but little from that of the objectives by Grubb, and under this assumption the foci for rays having the wave-lengths 495 and 486 $\mu\mu$ (F) would differ respectively from the focus for λ 500 by 0.00010 and 0.00025 of the focal length, or in the case of the Lick telescope, by 1.7 mm. and 4.3 mm. Placing the circle of aberration for $\lambda\ 500 = 0$, computation gives for the circles of aberration of the rays λ 495 and λ 486 the values 0.09 mm. $=$ 1″.1 and 0.23 mm. $=$ 2″.8 respectively. Remembering now that the spectrum of the Nova at that time was discontinuous, consisting of a line at λ 500 with intensity 10, a second at λ 495 with intensity 3, and a third at λ 486 with intensity 1,§ it is evi-

* A. N., 3119.

† *Monthly Notices*, R. A. S., Vol. LIII, No. 3.

‡ Monatsber. der Königl. Akad. der Wiss. zu Berlin, April, 1880; Publ. des Astrophys. observatoriums, No. 14.

§ Publications A. S. P., Vol. IV, p. 245.

dent that with the focus adjusted on the brightest rays λ 500, a stellar point would be seen, snrrounded by a circular halo about 1″.1 in diameter, which with less magnification would blend with the star, and around that a second bluish halo about 2″.8 in diameter. In a telescope without chromatic aberration, *i. e.*, a reflector, on the other hand, all rays would be united in a single point, and the Nova would appear as a star.

The other violet lines in the spectrum of the Nova are very faint in comparison with the line at λ 500, and give such large circles of aberration that the latter, from their faintness, would be invisible. The group of lines the brightest of which (intensity 0.7) has the wave-length $463\mu\mu$, would give a circle of aberration 12″ in diameter, and the line at λ 436 (intensity 0.8) a circle 24″ in diameter. The above considerations and a note by Barnard in *A. N.* 3114 and 3118, appear to confirm my view that the observed nebulous envelopes of the star are nothing more than circles of chromatic aberration. The note reads " the nebulosity, which was pretty bright and dense, was found, by the micrometer to be 3″ in diameter. Surrounding this was a fainter glow perhaps half a minute in diameter." In the Pulkowa refractor the appearance was quite the same.*

Further confirmation of the correctness of my explanation is found in notes on the observations themselves. Barnard says; † "October 25: I do not think the nebulosity has decreased in extent. November 4: The nebulosity and nucleus are bluish white. November 18: Nebulosity dense and bluish. . . An inspection of these notes shows that in determining the brightness of the Nova in its present condition much will depend upon the telescope and magnifying power. With a low power on a telescope inadequate to show its true nature the Nova is brighter than star F. With a high power on an instrument capable of showing it well, the star itself (or more properly, nucleus) is decidedly fainter than star *F*."

With a high power the circle of aberration for λ 495 appears distinctly separated from the star; the star formed of rays of wave-length $500\mu\mu$, is estimated to be fainter than when, with a lower power, the circle and star blend together from insufficient magnification,

It appears from the following notes, moreover that the Nova at its first appearance as seen in the Lick instrument differed from

* The same explanation of the appearance of the Nova in large refractors was also given by Newall (*Nature*, Nov. 3, 1892). See also Astro-Physical Notes in this journal, February, 1893.—*Tr.*

† *A. N.* 3143.

other stars according to the prominence of the bright lines as compared with the continuous spectrum. In this case, however, the difference was less marked, because several lines occurred between C and F, and the relative intensities of the lines were different. "1892, April 4: Nova is somewhat nebulous." "Nova seems to be fuzzy at times. Is it in focus when the other stars are?"* The last note is particularly convincing, for it is evident that the focusing was difficult because the differences of brightness of lines were not very great, while in the present spectrum, in which the line λ 500 dominates, the focus can not be well adjusted except on the point where rays of this wave-length are united.

III. *Hypotheses in Regard to the Nature of the New Star.*

In spite of the inconsiderable brightness of the new star, the application of improved instrumental means, and particularly of astronomical spectrum photography, has resulted in a collection of observational material so rich that observations of all previous occurrences of the same character appear poor by comparison.

A considerable advance in our knowledge of these celestial phenomena is therefore to be expected, and in my opinion it will be found to be substantially this: that we can no longer regard the assumption of a single body as sufficient in any explanation of the occurrences which we are considering. Although in earlier cases it was possible to advance hypotheses which were sufficient to explain the imperfect observations, and particularly appearances which, owing to unfavorable circumstances, were never clearly developed (for it should not be forgotten that it is to be regarded as an especial piece of good fortune that in this case the components of the motion of the bodies in the line of sight were great enough to allow their spectral lines to be separated), the real cause of the sudden, tremendous catastrophe, to which was ascribed the outburst of incandescent gas from the interior of a partly cooled body, remained hidden in total obscurity. It is therefore easy to understand why, among the numerous attempts which have been made to explain the phenomena presented by Nova Aurigæ, the assumption of a single body occurs in only quite sporadic cases.

I believe that I am justified in passing over all attempts at explanation which rest upon such an assumption as this; for exam-

† Publications A. S. P., p. 228.

ple the suggestion that a celestial body has suddenly erupted gaseons matter from its interior with such violence (of course in a direction as nearly as possible opposite to the Sun), that the gaseous matter has been thrown off from the body, and that the two parts are separating with a relative velocity of more than 460 miles per second. I will also merely state in the words of the author himself, a hypothesis of Sidgreaves (whose excellent observations I have mentioned in a previous section), since I have not succeeded in forming a perfectly clear picture of the action he conceives to have taken place: "The widening of the lines must be attributed to circular velocity in a plane or planes not greatly inclined to our sight line, and the advancing parts of the whirling gases must be covered by a sufficient depth of absorbing medium to give the dark bands. A great cyclonic storm of heated gases rushing towards us in the lower atmosphere of the star, trending upwards and returning over the star's limb in the higher regions, would satisfy all the requirements of the spectrum, and might meet with favor if only we could accept the form of disturbance, the high velocities and six weeks' (?) duration as probabilities. But if we estimate possibilities in the heated atmosphere of a giant star by the velocities and durations of some of the destructive cyclonic hurricanes in the cold atmosphere of our little Earth, we can hardly deny possibility to this origin of the spectrum."*

Lockyer† sees in the phenomena of the Nova a confirmation of his meteoritic hyphothesis, and explains them as the result of the collision of two meteoric swarms, a somewhat dense swarm, moving in the direction of the Earth with great velocity, passes through a less compact swarm moving in the opposite direction. But why all the particles of the denser swarm, or at least most of them should give spectra with dark (absorption) lines, and the particles of the sparse swarm, for the most part, spectra with bright lines, is not further explained; nor is the question investigated, how the enormous relative velocity of over 460 miles per second can persist after the mutual penetration of two cosmical clouds or meteoric swarms, involving the close passage and inevitable collisions of particles whose masses are of the same order and the transformation of their energy of motion into heat.

More carefully considered, and more in accordance with probability and the observed facts are the views which Huggins‡ has

* Memoirs of the Royal Ast. Soc., Vol. LI, p. 34.
† Proc. Roy. Soc. Vol. 50, p. 435.
‡ Proc. Roy. Soc., Vol. LI., p 493.

advanced. He starts with the assumption that two bodies exist, which are either gaseous or provided with gaseous atmospheres, and that these bodies, after approaching very closely, are moving in parabolic or hyperbolic orbits, whose axis lies nearly in the direction of the Sun. It is readily conceivable that the components of the motions of the two bodies in the line of sight, after their passage, should be as great as those observed in the Nova, and also that the velocity should change but little in a long time. Unfortunately we have no information with regard to the motion at the time of the occurrence which caused the sudden outburst of the star, since the first spectrum observations were made some forty days after this event.

Huggins remarks further, that the close approach of the two bodies might be regarded as a periodical disturbance repeated at long intervals, as in the analogous hypothesis relating to long-period variables, but that in the case of the Nova the great velocities of the components seem rather to indicate that they are not mainly due to the mutual attractions of the bodies, and the assumption is preferable that two bodies, already possessing great velocities, have accidentally met. In any other case one is compelled to assume masses which are enormously great in comparison with the Sun. The same result was also reached by Seeliger (A. N. 3118), who made the relations just considered the subject of a more elaborate computation.

A direct collision of two celestial bodies is not regarded by Huggins as an admissible explanation of the Nova; a partial collision has little probability, and the most that can be admitted is perhaps the mutual penetration and admixture of the outer gaseous envelopes of the two bodies at the time of their closest approach. A more probable explanation is given by a hypothesis which we owe to Klinkerfues, and which has more recently been further developed by Wilsing, *viz.*: that by the very close passage of two celestial bodies enormous tidal disturbances are produced and thereby changes in the brightness of the bodies. In the case of the two bodies which form the Nova, it must be assumed that these phenomena are displayed in the highest degree of development, and that changes of pressure have been produced which have caused enormous eruptions from the heated interior of the bodies; the eruptions are perhaps accompanied by electrical action, and are comparable with the outbursts in our own Sun, although they are on a much larger scale.

In such a state of things, all the conditions for the reversal of spectral lines, and for subjecting them to continual changes, are

fully developed, and since such relations are apparent in the bright and dark lines in the spectrum of the Nova, it can hardly be denied that the above assumption has ample justification.

Huggins is of the opinion that the luminous source which gave the continous spectrum, crossed by dark lines strongly displaced toward the violet, always remained within an envelope of cooler absorbing gas, forming with the latter the body which approached the Earth. The reason that the receding body emitted bright lines, while the approaching body gave a continuous spectrum with dark bands, Huggins believes is to be found in the different stages of development of the two bodies, and consequently in the accompanying diversity of density and temperature which must exist in them.

Finally, Huggins directs attention to the variations of light which took place in the beginning, and the rapid diminution of light which followed; also to the fact that the spectrum showed no changes in the relative brightness of the principal lines as long as they could be observed. Here also he finds support for the views which he holds. After several oscillations, the tidal disturbances were followed by a state of quiescence, the outer and cooler gases again completely enclosed the bodies, and the transparency of the atmospheres diminished as the distance between the bodies increased.

The doubts which oppose themselves to these views of Huggins, and to all similar hypotheses, arise mainly in the improbability of the meeting of two bodies moving in opposite directions with such abnormal velocities. If, with Huggins, we regard the broad, bright lines as single (broadened) lines, and the maxima which appeared in them as phenomena of reversal, the displacement of the middle of the lines as compared with the corresponding lines of the terrestrial source is easily determined, and the result is a motion of about 275 miles per second away from the Sun for the one body, and a motion toward the Sun of about 460 miles per second for the other. If we consider further that these are only the components of motion which lie in the line of sight, and that the real motions may be much greater, the improbability becomes even greater than before.

The fine, bright lines which appear in the dark lines of hydrogen, I have regarded as reversals from the first, but to refer the maxima of intensity in the bright lines to a similar cause seems to be hardly justifiable according to my observations, and this constitutes the second objection which I have to make to the views advanced by Huggins. In the normal course of develop-

ment of reversal phenomena in bright lines, a narrow dark line first appears in the middle of the strongly broadened bright one; this broadens with increasing density of the vapor, and a fine bright line appears in its center, constituting a double reversal. It may be that an unsymmetrical disposition of the parts with respect to the middle of the reversed line, and unequal intensities, may occur; but I have never observed them, even when the lines were much more strongly widened than the bright lines in the spectrum of the Nova.

Now all observers agree that the distribution of light in the bright lines of the Nova, with respect to the middle of the lines, was decidedly unsymmetrical, and that this aspect of the lines did not change materially throughout the entire time of the first apparition. It would therefore be necessary to suppose that the component of the Nova which gave the bright-line spectrum did exhibit such an unsymmetrical formation of lines, and a peculiar abnormal distribution of light in them.

Finally, the objection can still be made, that sensible tidal action cannot be assumed to last for any considerable time, as on account of the great relative velocity of the bodies, they would separate at the rate of 46 millions of miles per day. Seeliger* proves that considerable tidal disturbances could in fact have lasted but a few hours. It should not be forgotten, however, that a whole chain of phenomena and oscillations of the most gigantic kind, which might endure for weeks and months, would follow these disturbances, and it is from this point of view that I believe Huggins' hypothesis should be interpreted.

Belopolsky† gives his views in regard to the Nova in the following paragraphs; "As an explanation of the whole phenomenon there remains only the assumption that we have to deal with the superposed spectra of two or more bodies. One body, with an extensive hydrogen atmosphere, and relatively low temperature was approaching us with enormous velocity, while a second, whose temperature was high, and in whose spectrum the bright lines of hydrogen appeared, moved with a velocity which perhaps varied during the time of observation, so that the body first receded from, and then approached us."

"The latter body perhaps consisted of several smaller ones moving in directions variously inclined to the line of sight. The constancy and enormous magnitude of the velocity of the first

* A. N., 3118, ASTRONOMY AND ASTRO-PHYSICS, Dec., 1892.

† Spectrum der Nova Aurigæ, Mèlanges mathèm. et astron. T VII, St. Pètersbourg, 1892, p. 297.

body allows the conclusion to be drawn that it was the chief body of the system, and that its velocity is to be ascribed to initial momentum, and not to the attraction of other bodies. The second body (or system of bodies) was then the one that was rendered incandescent in the atmosphere of the first. In comparison with the first, its mass was smaller, and hence by its passage through the atmosphere of the first, a sufficient quantity of heat might have been evolved to convert it into glowing vapor. The occurrence must have been analogous to the explosion of a meteor in the atmosphere of the Earth, (or to a comet at perihelion), whose small mass is rendered incandescent, and reduced to glowing vapor, without making luminous the atmosphere itself."

"This small mass probably described a hyperbolic orbit around the principal body. After it passed out of the gaseous envelope of the latter its brilliancy would be rapidly extinguished as we saw was actually the case. A secondary brightening is often observed in meteors and comets, as well as a continual fluctuation of light during the latter part of their period of visibility."

It is not immediately clear how Belopolsky arrives at the assumption that the body with bright lines in the spectrum was perhaps moving away from us at the beginning of the observations, and afterward toward us. This assumption is, however, a consequence of the supposition that the amount of the displacement of the bright lines cannot be correctly determined, on account of the unsymmetrical form, due to the presence of the dark lines, and it is based upon a small change which Belopolsky observed in the intensity curve of the bright $H\gamma$ line between the first three and the last three observations.

[*To be Continued.*]

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

Monsieur Cornu's Spectroscopic Investigations.—A visit to M. Cornu's laboratory at the Ecole Polytechnique in Paris is full of interest to the spectroscopist. Much has been written from time to time on the so-called "secondary" spectrum of hydrogen, and the difficulty of obtaining the lines of Balmer's series in the vacuum tube is well-known. Professor Cornu's earlier photographs of the ultra-

violet hydrogen spectrum show a very large number of lines, and it is difficult to distinguish in them the lines of Balmer's series. The most elaborate precautions were then taken to obtain the hydrogen in a state of absolute purity. The gas was produced by electrolysis, and carefully dried and purified before being admitted to the vacuum tube. The mercury pump (Sprengel) was separated from the vacuum tube by drying media and vessels filled with copper and sulphur. The pump was frequently cleaned with ozone, and sparks were allowed to pass through it from time to time. In the vacuum tube the electrodes ordinarily employed gave place to exterior metallic sheaths. The result of all these precautions was most satisfactory. The "secondary" spectrum almost entirely disappeared, leaving the lines of Balmer's series strong and well-defined in the photographs. The rythmical grouping of the lines is noticeable at a glance, and no one would hesitate for a moment in selecting the members of the series.

M. Cornu's collection of photographs showing the reversal of metallic lines in the electric arc and spark is very extensive, and the fidelity with which certain phenomena of reversal well-known in the Sun repeat themselves in the arc is most striking. G. E. H.

Spectroscopy at Stonyhurst College Observatory.—Even to the casual visitor the beautiful old college at Stonyhurst holds out a wealth of attractions, but to the astro-physicist a day spent on this Lancashire hill-side is a pleasure indeed. During a recent visit we were fortunate enough not only to examine the extensive buildings of the College under the guidance of Father Sidgreaves, but also to make a careful inspection of the work of the Observatory. In addition to magnetic and meteorological observations, this consists of researches in solar and stellar physics. Photographs of the spectra of various parts of the solar surface—particularly of spots and faculæ—are made with a large spectrometer which has been fitted by Hilger with quartz objectives and a camera tube. An image of the Sun is formed on the slit by an objective receiving light from a heliostat placed on the roo of the Observatory Cords within easy reach of the hand of the observer give him complete control of the heliostat. The dispersion piece of the spectrometer is an excellent Rowland grating. The sharpness of the photographs leaves nothing to be desired. The double reversals of the H and K lines in faculæ are shown in great perfection, and Father Sidgreaves has even succeeded in obtaining faint double reversals of these lines in the general light of the Sun *i. e.*, when sunlight reflected from the heliostat is received upon the slit without the interposition of a lens. The Sun is hence a "bright-line star," though hardly so in the ordinary sense of the term, for if it were removed from us to the distance of the nearest star, its light would be so enfeebled as to render the detection of the bright lines almost, if not quite, impossible.

In stellar spectroscopy Father Sidgreaves' most important investigation is fully described in his well-known Memoir (published by the Royal Astronomical Society) on the spectrum of *Nova Aurigæ*. The spectroscope used has two direct-vision prisms, with collimation and short photographic telescope, and has been used in conjunction with an 8-inch refractor. The novelty of the arrangement is in the absence of a slit, the star being made to drift slowly in a direction parallel to the refracting edges of the prisms. The resulting spectra are surprisingly sharp, and the exposure is much less than that required with a slit spectroscope of the same dispersion. By inclining the photographic plate, rays having their foci at different points along the axis of the refractor are focussed simultaneously. Curiously enough, it has not been found that the definition of spectral lines is materi-

ally affected by scintillation. A circumpolar star photographed when near the horizon and again at its upper culmination gave equally sharp lines in the two cases.

The tube of the 8-inch refractor has recently been enlarged and lengthened to fit it for a 15-inch objective just completed by Sir Howard Grubb. The mounting has fortunately proved itself capable of carrying the increased weight, and the instrument in its improved form will stand as a most suitable memorial to the late Father Perry. In the able hands of Father Sidgreaves the greatly increased light-grasping power of the telescope will be of important service in his photographic investigations of stellar spectra. G. E. H.

Professor Lockyer's Researches on Stellar Spectra.*—The well-known opinions which Professor Lockyer holds with regard to the origin and constitution of the different classes of heavenly bodies have been largely founded on the observations of others, as his own work has until quite recently been confined to the sun and to laboratory investigations. During the last two years, however, spectroscopic observations of the brighter stars have been carried on under his supervision at Kensington, and the principal results are embodied in a memoir which has just been published in the Philosophical Transactions of the Royal Society. The observations are discussed with special reference to their bearing on Professor Lockyer's meteoritic hypothesis, the objects of the inquiry, as stated in the memoir, being as follows:—

(1). To determine whether the hypothesis founded on eye observations is also demanded by the photographs.

(2). In the affirmative case to discover and apply new tests of its validity, or otherwise.

The reader of the memoir will naturally look with interest for Professor Lockyer's deductions. For a complete statement of the argument the paper itself must be referred to, but some of the matters which enter into the discussion are considered below, not perhaps in all cases, on account of their bearing on the main line of the argument, but because they have been the subjects of more or less controversy.

Various instruments were employed in the investigation, the most effective form being the object-glass spectroscope originally used by Fraunhofer, and recently revived with so much success at Harvard College Observatory. The largest of these instruments had an aperture of ten inches; other telescopes with six inches aperture were however more frequently used, and some photographs were taken with a spectroscope having a slit and collimator. In enlarging the original negatives, the breadth was increased by a device quite similar to that used by Dr. Scheiner, the only difference being that the reciprocating motion across the length of the spectrum was given to the negative instead of to the plate on which the enlargement was made. While it is doubtful whether there would be any appreciable difference in the results, if the apparatus in each case was well constructed, the advantage, if any, would seem to lie with the arrangement of Dr. Scheiner, as with his construction any irregularity of fitting or motion of the slides would have less effect on the enlargement. The method employed at Kensington had been in use for some time before Dr. Scheiner's method was announced.

In all, 443 photographs of the spectra of 171 stars were obtained. As the

* On the Photographic Spectra of some of the Brighter Stars. By J. Norman Lockyer, F. R. S., Philosophical Transactions, Vol. 184 (1893), pp. 675-726.

object was to secure detailed spectra of a comparatively few stars rather than the spectra of many, several stars were photographed quite a large number of times. Some greatly enlarged and highly successful photographic reproductions of selected spectra are given in five large plates which accompany the memoir.

The material thus obtained by observation was then classified, the stars being arranged in four tables (A, B, C, D), with reference to the amount of continuous absorption in the upper part of the spectrum. Each table contains numerous subdivisions. These different classes are afterwards rearranged with reference to their places in the order of development according to the meteoritic hypothesis, giving rise to a somewhat involved notation that considerably embarrasses the reader, who is under the necessity of frequently comparing the different tables. The stars considered in the memoir are included in the first three of Secchi's types, those in table D (bright line stars and Nova Aurigæ), being reserved for a future discussion. Bright-line stars of the Wolf-Rayet type and stars of Secchi's type IV, were unfortunately beyond the powers of the appliances at Kensington.

It will, of course, be remembered that Professor Lockyer's system of stellar classification provides for both an ascending and descending branch of the temperature curve, and in this respect it certainly has advantages over other systems which claim to have a rational basis. It is therefore interesting to observe that the photographed spectra are found to bear out the interpretation of the visual observations on which the classification was founded, at least, in the types of stars to which the photographs are restricted. On attempting to arrange the spectra in regular sequence, "one important fact comes out very clearly, namely, that whether we take the varying thicknesses of the hydrogen or of the lines of other substances as the basis for the arrangement of the spectra, it is not possible to place all the stars in one line of temperature. For example, in the stars of table A, sub-division α, the hydrogen lines are of the same average thickness as in the stars of Table A, sub-division β, but the remaining lines are almost entirely different; and the two sub-divisions cannot be placed in juxtaposition. It is, therefore, necessary to arrange the stars in two series."

In the tabular arrangement which follows, α *Andromedæ* is the representative of the highest temperature conditions. In tracing the sequence of increasing temperature, it is found that "From α *Herculis* to α *Andromedæ*, we thus have a continuous series of spectra, the dark flutings first disappearing, and afterwards most of the lines of the more common metals such as iron and manganese, lines of unknown origin gradually replacing them. At the same time, the amount of continuous absorption is gradually diminishing, and the lines of hydrogen are increasing in intensity."

In the same way there is found to be a perfectly continuous series of spectra from α *Andromedæ* to *Arcturus*, on the descending branch of the temperatur curve.

It is evident that it must be a very delicate matter to decide whether a star belongs to one or to the other branch. "The difference between these (lines in stars of decreasing temperature) and the lines seen in stars of increasing temperature should be one due to the different percentage composition of the absorbing layers, so far as the known lines are concerned." The difference in the appearance of the spectra would therefore be small, and the classification would hardly be possible without such a long series as was obtained at Kensington.

The existence of carbon absorption in stars near the middle of the descending temperature branch has an important bearing on the meteoritic hypothesis, but

in general it could not be studied in these spectra, as they did not extend sufficiently far into the violet for the purpose. In the case of *Arcturus*, however, the only star in which this region was photographed, the solar carbon band at λ 3883 was apparently present, but had nearly the same intensity as in the solar spectrum.

With regard to the presence of carbon in stars of Secchi's third type, (placed by Lockyer in Group II, with ascending temperature), the evidence of the photographs does not seem to be in favor of that important part in determining the character of the spectrum which it is assumed to have in the meteoritic hypothesis. In the spectra of *α Herculis* and *α Orionis*, photograped on isochromatic plates, the bright edge of a fluting agrees closely with the brightest edge of the carbon fluting at λ 5165, but the coincidences of bright places in the star spectra with the two secondary maxima of the same fluting are more than doubtful in the case of *α Herculis*, (the discrepancies amount to about eight tenth metres), and apparently no bright places corresponding to them exist in the spectrum of *α Orionis*. It is possible, as Professor Lockyer suggests, that the secondary maxima may be masked by some of the dark lines which appear in the same region. Other flutings of carbon are not represented in the spectra of these stars, but there is said to be strong evidence of the existence of the carbon group commencing at wave-length 4215.6, in a photograph taken by Professor Pickering. Although the evidence, taken as a whole, will probably appear pretty slight to the reader, Professor Lockyer considers it sufficient to establish the presence of bright carbon flutings in these stars.

In all the photographs of stars of this type taken at Kensington, the sharp edges of the dark bands have the same positions. The slight differences found by Dunér and Vogel are to be ascribed to the difficulties of eye observation, and perhaps, in a small degree, to differing velocities in the line of sight.

The iron lines in the spectrum of *α Orionis* are intermediate in character between the lines of the arc and those of the flame spectrum, and the inference as to the temperature of the regions in which the absorption takes place is obvious.

Stars of Secchi's fourth type could not be photographed at Kensington, and for reasons already given, the bright-line spectra which were obtained are not considered in the memoir. There is, however, a very clear statement of the appearances to be expected on the hypothesis, as compared with the appearances actually observed, which covers the whole range of stellar development. In this some modifications of former views will be noticed. Thus, the explanation on p. 710 of those lines (D_3, λ 4471) in the spectra of the nebulæ which are considered to indicate a high temperature,—namely that they are due to the relatively small number of end-on collisions among the meteorites of the swarm,—must be regarded as very much more satisfactory than the vague suggestion that "the degree of fineness which is brought about by temperature in the case of the Sun, is brought about in the spaces between meteorites by extreme tenuity." (*Proc. Roy. Soc.*, Vol. 48, p. 139, 1887). We find also an explanation of the reversal of the hydrogen lines in such stars as *Pleione*.

Professor Lockyer is reluctant to abandon the magnesium origin of the chief nebular line, while holding that it is not of fundamental importance in his argument. Commenting on the observations made at the Lick Observatory on the character and position of this line, he says, "The Lick telescope is perhaps the ideal telescope *not* to employ in such an inquiry." This is doubtless a survival of the idea that a short-focus telescope gives brighter spectra than a long-focus one, and illustrates the tenacity of life in a fallacy which has once obtained wide-

spread belief. The brightness of the spectrum as seen in a spectroscope cannot be determined by the brightness of the image on the slit-plate without considering the dimensions of other parts of the instrument, the true criterion of brightness being the effective aperture of the spectroscope; thus the spectrum obtained with the long-focus telescope may very likely be the brighter. The longer telescope has moreover an advantage, which, as it involves a physiological effect, cannot be expressed mathematically, but which is nevertheless of very considerable importance; and that is the larger scale of the image on the slit-plate, and consequently greater length of the lines seen by the observer. This advantage is obviously greatest for small objects, and is reduced to nothing for objects exceeding a certain (indeterminate) angular magnitude. As a matter of fact, the Lick telescope as much exceeds in precision any instrument which had been used for the same class of observations, as the meridian circle exceeds the sextant.

A more important question, with reference to its bearing on the metroritic hypothesis, is the presence of bright carbon flutings in the spectra of nebulæ and bright-line stars. For stars of the Wolf-Rayet class it seems to have been pretty definitely settled in the negative; for other stars the existence of carbon radiation cannot be said to have obtained general acceptance; but Professor Lockyer has reserved this branch of the inquiry for another paper, which will be looked for with interest.

Some other points brought out in the memoir may still be mentioned. Fine lines appear in the photographic spectrum of α *Aquilæ*, in addition to the particularly hazy lines which are most conspicuous; hence the explanation suggested by Professor Pickering (*Annals of Harvard College Observatory*, vol. 26, p. 21), that the haziness is caused by a very rapid rotation of the star, must be rejected. A comparison of photographs of the spectrum of α *Tauri* taken at Harvard College Observatory and at Kensington shows some evidence of slight changes in the relative strength of the lines. The presence of the series of hydrogen lines in the ultra-violet is not in itself evidence of a very high temperature. Cornu has obtained the complete series of lines with an ordinary spark without jar. The high temperature of such a star as *Sirius* is not indicated by the fact that its spectrum shows the whole series of hydrogen lines, but by the fact that there is bright continuous radiation far in the ultra-violet.

It will be observed that the stars considered in the memoir, or rather the stars whose spectra have actually been photographed at Kensington, are those which present the fewest difficulties to other systems of classification. The system of the meteoritic hypothesis differs from these mainly in assigning an increasing temperature to some stars and a decreasing temperature to others. That such a distinction can be made, Professor Lockyer regards as established by the photographs as well as by visual observations. The general result is thus stated:

"The general result of the above discussion then, as far as it goes is, as follows: —Among the 171 stars already considered there are really two series of spectra, one representing the changes accompanying the increase of temperature, while the other represents the effects of decreasing temperature. The fundamental requirement of the meteoritic hypothesis is, therefore, fully justified by the discussion of the photographs."

In accordance with this conclusion, therefore, the "Extension of the Original Classification" which concludes the paper contains no important modification of the author's previous views. J. E. K.

New Stars.—Some fifteen years ago Professor Bickerton published in the "Transactions of the New Zealand Institute" a monograph bearing on the origin of new stars, which may throw some new light on the outbursts last year in the constellation of *Auriga.* He there attributes new stars to a "grazing'" collision of stars like the sun. The temperature developed is independent of the amount of grazing, and in similar substances depends only on the velocity destroyed, so that the resultant body would be as if the whole sun collided. An intensely brilliant body may be produced for a few hours, the high molecular velocity of the intensely hot body quickly carrying off the particles into space beyond the influence of the mass's gravity. If rather more than a "grazing" collision occurs, the molecular velocity will be the same, but the mutual attraction greater, and thus a hollow globe of gas, or planetary nebula, will be formed.—*Journal of the British Astronomical Association,* Vol. III, No. 11.

New Variable Stars.—According to Wolsingham Circular No. 37, an anonymous red star, observed at R. A. $19^h\ 7^m\ 16^s$, $+ 25°.46$, (1855), is variable. Its magnitude was 9.0 on Aug. 21, and had diminished to 11 on Nov. 14. Photographs taken with the 8-inch Compton telescope confirm the variability of Espin 329, (R. A. $19^h\ 59^m\ 6^s$, Dec. $+ 36°.25$.)

Dr. Anderson announces that D. M. $+ 26°.43$ is variable. The range of variability seems to be small, the estimates of brightness at different times ranging from mag. 9.0 or 9.2 in the D. M. (1855) to 8.7 in 1878; but another observation at Bonn in 1855 makes it 8.3. The place for 1894 is R. A. $0^h\ 16^m\ 51^s.3$ Dec. $+ 26°\ 24'\ 27''$.

New Nomenclature for the Hydrogen Series.—Hitherto the lines of hydrogen forming Balmer's well known series have been referred to under various names. The lines called by Fraunhofer C and F are rarely given other titles, but the line near G is frequently called $H\gamma$. The remaining line in the visible spectrum is usually known as h, while that near H (calcium) has been designated $H\varepsilon$. The Greek subscript α was applied many years ago to the ultra-violet member of the series just above H and K, and the remaining lines in the "stellar series" were denoted by subscripts formed from the successive letters of the Greek alphabet. The duplication of names has naturally given rise to some confusion, and it is satisfactory to learn that Drs. Huggins and Vogel intend in the future to call the hydrogen line coincident with C, $H\alpha$, that at F, $H\beta$, and so on through the entire series, thus doing away with the meaningless distinction between the visible and ultraviolet lines. We trust that all astrophysicists will unite in the adoption of this new and convenient system. G. E. H.

New Scientific Terms Wanted.—Professor Newcomb, writing to *Nature,* points out the desirability of having some short and convenient word to express the entire radiation of a body without reference to the manner in which the existence of different rays is made evident. As the now universally accepted view, that the only difference of kind in the rays of the spectrum are those of wave-length, is of quite recent origin, a fitting word has not yet been invented. "Radiant energy," the most accurate of the expressions in use, is subject to the objection of being a description rather than a name. The word "light" is properly applied only to ether waves whose lengths fall between certain limits, and there is no corresponding word for other waves; hence its use although convenient, is unscientific.

Professor Newcomb suggests the term "radiance" as one well fitted to supply

the want in question, and thinks that "The vague and poetic idea hitherto associated with it is an advantage, because it enables us to adapt it to the case in hand with greater readiness than we could adapt a word which already had some well-defined meaning."

To "radiate" would then mean to emit radiance, "radiometry" would mean the measure of radiance, etc. Terms corresponding to transparency and diathermancy would be transradiant or transradious.

The suggestion of Professor Newcomb seems to be an excellent one. We have already many words, such as "energy" with a strictly definite meaning in science, and a much wider one in ordinary use. The admission of radiance to this class would be a great convenience to writers.

M. Deslandres, in an article on the solar faculæ in *Knowledge*, proposes the use of the term "flammes faculaires" for the masses of calcium vapor which are found over faculæ and produce the double reversal of the K line. Mr. Ranyard objects to the word "flame" as seeming to imply chemical action and therefore tending to beg the important question whether chemical changes producing light and heat are going on upon the Sun.

Refractive Index of Liquid Air.—The recent work of Professors Liveing and Dewar, on the refractive indices of nitrogen and air, is both novel and interesting. The skill which these gentlemen have shown in handling substances near the absolute zero is well known. They find for liquid air the refractive index

$$\mu = 1.2062:$$

for liquid nitrogen at $-190°$ C, they get

$$\mu = 1.2053.$$

The method employed was that of total reflection as devised by Terquem.

These results furnish an interesting verification of the law of Gladstone and Dale that

$$\frac{\mu - 1}{d} = \text{constant}.$$

where d is the density of the substance. Mascart's value of this "refraction constant" derived from measures on nitrogen *at atmospheric pressure is* 0.237 while for *liquid* nitrogen Liveing and Dewar find

$$\frac{\mu - 1}{d} = 0.225.$$

This constant, it will be remembered, has a remarkably simple interpretation. It represents probably an optical constant of the gaseous molecule, and a function of the refractive index of the molecule.

The values of μ given above are each for the D line. The next problem which these distinguished workers have assigned themselves is to determine "the dispersive powers of liquid oxygen, nitrogen, and air at a temperature as low as $-200°$ C."—*Phil. Mag. Oct. 1893.*

Spectroheliograph for the Yerkes Telescope.—As the 40-inch Yerkes telescope is to be used during the greater part of the time for astro-physical investigations, three spectroscopic attachments are to be provided for it. Of these the star spectroscope has been completed by Mr. Brashear, and the construction of the solar spectroscope and spectroheliograph will shortly be undertaken. The solar spectroscope will differ from existing instruments of the same class mainly in its increased size and in certain novelties of construction. In the nature of the

case the general design of such a spectroscope must follow certain well-known conditions, which do not admit of wide deviation from existing types. But the designer of a spectroheliograph has more freedom of choice. The instrument is susceptible of decided variation in form, as I have already had occasion to point out. The various types of this instrument described in my paper entitled "The Spectroheliograph" (see ASTRONOMY AND ASTRO-PHYSICS, March, 1893, p. 241)* have each certain advantages and disadvantages, and these must be carefully weighed against one another in designing a large and important instrument for a special purpose.

The 40-inch Yerkes telescope has a focal length of about 64 feet, and will consequently give a solar image nearly 6.5 inches in diameter. A moment's consideration of the facts presented in the paper just referred to will make clear the impracticability of constructing a fixed spectroheliograph with moving slits large enough to allow the whole image to be photographed on a single plate without loss of light near the limb. Suffice it to say that the telescopes of such a spectroheliograph would have an aperture of about 9 inches. In spite of the great weight of such an instrument this large aperture would not of itself be an insuperable objection, were it not for the fact that the ruled surface of the grating (or the edge of base of the prisms, in case they were chosen) would be between 12 and 18 inches long. Unfortunately we have yet to see gratings or prisms (of large angle) of these dimensions.

Nevertheless, it was deemed of the utmost importance that the entire extent of the sun-spot zones be secured on a single plate. That is, it is desirable to photograph an equatorial zone about 4 inches in width. Manifestly this could not be done with a fixed spectroheliopraph (like that successfully used with a 2-inch solar image at the Kenwood Observatory) provided with slits moving in the focal planes of the telescopes. Hence the form of spectroheliograph described in the last paragraph on page 256 of the article already referred to has been adopted, substituting, however, one, two, or three simple prisms, with or without a plane mirror, for the grating and plane mirror there described. The arrangement of the prisms and mirror is such as to give a deviation of 180° for the K line. The collimator and telescope of the spectroheliograph are placed with their axes parallel, and a straight slit is fixed in the focal plane of the collimator objective. A similar slit in the focal plane of the telescope is of the same curvature as the K line, upon which it is accurately set by a screw moving the slit-plate in the plane of dispersion. The collimator and telescope are of equal aperture and focal length. The entire instrument is moved at right angles to the optical axis of the 40-inch telescope on wheels with ball-bearings, running on knife-edges. The frame which carries the knife-edges is attached to the equatorial by means of four steel tubes 4 inches in diameter. Provision is made for rotating the frame in position-angle, so that the motion of the spectroheliograph can be made parallel to the solar equator. The photographic plate-holder is fixed to the frame, and the second slit moves close to the surface of the stationary plate.

The most important advantages of this form of spectroheliograph is the large field photographed. The *length* of the field is evidently determined by the length of the knife-edges on which the instrument runs. This may conveniently be as much as ten inches, or even more. The *width* of the field is determined by the length of the slits, the *height* of the prisms, and the aperture of the collimator and telescope. For with the slit fixed in the axis of the collimator the *width* of

* The presence of the letter F in the second equation on p. 253 is due to a printer's error.

the illuminated portion of the collimator objective remains—with a given focal length—constant. It is always concentric with the objective. The base of the prism is thus comparatively small. For instance, if the ratio of aperture to focal length in the equatorial is 1-18, and the collimator is 36 inches long, the length of one side of a 60° prism would be about four inches. Prisms of this size may readily be obtained, and as an increase in the width of field means simply an increase in the *height* of the prism, and not in the size of base, a very large field can thus be photographed. In order to photograph in a single exposure, the 6.9 inch image given by the Yerkes telescope a collimator and telescope of 8.5 inches aperture and 36 inches focal length, with 60° prisms 4 inches on an edge and 5 inches high, would suffice. As such an optical combination would, to say the least, be extremely difficult to realize, it is probable that we must be content with photographs showing a zone not wider than four inches. G. E. H.

Mr. Campbell on the Accuracy of His Measures in the Spectrum of Nova Aurigæ. —As Professor Vogel, in his Memoir on Nova Aurigæ, expresses doubt whether the changes in the wave-lengths of the bright lines, as observed at the Lick Observatory, should be regarded as established Mr. Campbell, has published a note in A. N. 3192, giving in full the results of a series of measures of the three nebular lines, for the purpose of illustrating their accuracy. The table of measures which he gives shows that the probable error of observation was remarkably small. The comparisons of the three brightest lines of the star with metallic lines gave respectively velocities of — 55, — 51, and — 61 miles, referred to the Sun.

As for the possibility of constant errors, experiments showed that the observed change in the position of the lines was much greater than any error that could be made in estimating the middle of the broad line. The wave-length of the chief line in the spectrum of the Orion nebula, measured in the same way, and on the same night, was 5007.39, differing from Keeler's mean result by only 0.05 tenth-metres.

Mr. Campbell says of the results in his table: "It will be seen that the three lines observed in the Nova are almost equally displaced from the normal positions of the three lines in the nebulæ. In fact the results agree a little more perfectly than I expected they would. But I would undertake at any time to repeat these observations, with perfect confidence that the displacements of the two principal lines would differ from each other considerably less than one tenth-metre." The comparisons already referred to, of the changes in the wave-length of the line with the amount of error in estimating the position of its center, leave no basis for the suspicion that the observed changes of wave length might be due to variations in the distribution of the light within the broad line.

Professor Vogel on the New Star in Auriga.—In our translation of Professor Vogel's monograph, concluded in the present number, a considerable part of Section II is omitted, for the reason that it consists mainly of an abstract of observations which we have already printed in full. But Professor Vogel occasionally comments on the observations and makes his own deductions from them, and these, coming from so distinguished an authority, cannot fail to be interesting, particularly as they are sometimes at variance with the deductions of the observers themselves.

With regard to the observations of Dr. and Mrs. Huggins, Professor Vogel attaches considerable importance to the note that the displacement of the D line toward the red was not so great as that of the F line, although the observers regard it as having but little weight on account of the difficulty of the compari-

sons. Still more important is considered to be Dr. Huggins' observation of the composite structure of the supposed nebular lines after the reappearance of the star, and the contrast which these groups of lines presented both with flutings (magnesium) and with the fine sharp lines in the spectrum of the Orion nebula.

The observations of Pickering, Copeland and Becker, and Maunder do not need to be specially considered here, as they are not in disagreement with those made at Potsdam. The explanation of the Nova proposed by Lockyer is referred to in the translation.

In reviewing the observations of Belopolsky, Professor Vogel says, with reference to the fine lines found by that observer in the (first) spectrum of the Nova, and the assertion that no iron lines were present, "I must remark that the assertion that there were no iron lines in the spectrum of the Nova is in direct contradiction to the results which have been obtained by other observers and also by myself; I can also not admit that all other lines except those of hydrogen can be characterized as faint and fine. On the contrary I wish to assert that very many lines were quite similar in appearance to the hydrogen lines, and the observations of Huggins and Campbell are in accordance with this view."

The great amount of detail on the photographs taken at Pulkowa is regarded with suspicion by Professor Vogel, who says in this connection:

"I am constrained to remark, with reference to these observations, that the extraordinary wealth of detail on the spectrograms may have some relation to the extraordinarily long exposure of five hours which was given them, and which may have given rise to small displacements of the spectrum on the plate. According to my experience, displacements of this kind are perceptible even in exposures of one hour, and at temperatures under 0° C, their effects being to broaden the lines, or even easily to give rise to the impression of their being double."

Belopolsky's views on the character of the hydrogen lines are criticised as follows:

"Belopolsky is of the opinion that it is possible to speak of displacement in the case of the dark line Hγ only, as it alone has sharp edges and a symmetrical form. The bright Hγ line is unsymmetrical, which is no doubt to be ascribed to the adjoining dark line. 'The latter (the dark line) probably covers (verdeckt) the second edge of the bright line, and therefore we cannot decide whether the bright line is displaced or not, or determine the amount of the displacement, if there is any.' To this I must remark, that the view that in superposed spectra dark lines in one spectrum can occult (überdecken) bright lines in the other cannot be correct, for the dark lines are nothing positive, but only places of less luminosity in an otherwise continuous spectrum, and therefore cannot occult and extinguish the bright lines of another spectrum as dark, opaque bodies would, Aside from these considerations, the assumption that the bright hydrogen lines are unsymmetrical on account of the proximity of the dark lines is not valid, for a great number of broad bright lines in the spectrum of the Nova, which had no dark companions, were likewise sharply bounded on one side and diffuse toward the less refrangible end of the spectrum."

Having given Professor Vogel's criticism, it is only fair that we should devote the necessary space to Herr Belopolsky's reply. In *A. N.* 3184, at the end of an article on the motion of ζ Herculis in the line of sight, Herr Belopolsky says:

"The following circumstances vouch for the reality of the details on the Pulkowa plates. Since, at the appearance of the Nova, the spectrograph was

attached to the 15-inch refractor in a hasty and provisional manner, all the arrangements for keeping the star in the same part of the slit were defective; the driving-clock ran badly on account of the severe cold and imperfect balancing and the handles of the slow-motion screws in R. A. and Decl. were too short."

"For this reason three quite isolated spectra are found close together on the plate of March 2, each of which, therefore, received an exposure of 1 hour 40 minutes.* The details in all these spectra are the same; hence they are either real, or the errors of the instrument have affected each of the spectra in precisely the same way."

"Morever, the characteristic details of the Pulkowa spectrograms (resolution of the bands into lines) are to be found on the excellent photographs taken at Harvard College Observatory." . . .

"Finally, it is difficult to assume an error-producing influence that could leave the lines sharp in the spectrum of Venus, which was photograped on the same night, likewise at a temperature of from — 3° to — 15°, while it produced wide and doubled lines only in the spectrum of the Nova."

With regard to the presence of iron lines in the spectrum, Herr Belopolsky says:—

"I must remark, on my side, that all my assertions relate only to the small part of the spectrum included between wave-lengths 458 and 420, and I can only repeat here, that in this region none of the iron lines were to be found that had been photographed in the iron spectrum with the same instrument."

The rest of the article is as follows:—

"The remark of Professor Vogel's, that the dark line cannot cover the edge of the bright Hγ line, might be construed as a reproach for my ignorance of Kirchoff's law, if it were not that the mistake arose through a distinction, unknown on my part, (as I am not a perfect master of the German language), between the words 'verdecken' and 'überlagern.'"† . . .

"According to what Professor Vogel says, all bright lines should be diffuse on one side and sharply bounded on the other, whether they are accompanied by dark lines or not. This, however, is not seen on the photographs of Harvard College, where only those bright lines which have dark companions are sharply bounded on the side toward the violet."

"Finally, I must remark here, that the gaseous envelope of the Nova at its reappearance could be seen far better with the 15-inch than with the 30-inch refractor."

The beautiful photographs of Father Sidgreaves are next considered by Professor Vogel. In general, they accord with the results obtained at Potsdam. More than half of the 41 bright lines shown on these plates coincide with bright and frequently occurring lines of the solar chromosphere.

The following remarks are made on von Gothard's photographic comparison of the spectra of the Nova and various planetary nebulæ, (ASTRONOMY AND ASTRO-PHYSICS, January, 1893, and other journals):—

"To conclude, from the apparent agreement of the spectrum of the Nova with that of planetary nebulæ, that the spectra are identical, and further, that an object which is clearly enough shown by its spectrum to be a celestial body of stellar character whose surface has been strongly heated and then gradually cooled,

* This does not seem to follow, but the matter is unimportant.—Tr.

† Here some sentences are devoted to a comparison of passages by Professor Vogel and Herr Belopolsky in which these words are used,—words which in the translation have been rendered by "cover" and "overlap."

has changed overnight, so to speak, into a gaseous nebula, I consider to be at least very rash. However well convinced I may be of the value of Von Gothard's photographs, especially as they form so beautiful a supplement to the observations made at the Lick Observatory, I cannot agree with Herr von Gothard's views, when he characterizes his results as a most interesting and important discovery, and the change which the spectrum of the star underwent during the summer months, as unique in the history of astronomy. In contradiction of the last assertion particularly, I must recall the fact that the spectrum of Nova Cygni underwent a quite similar change. According to the observations of that time, when the powerful assistance afforded by photography was lacking, only a single line remained, which coincided with the brightest line of the nebular spectrum, λ 500.7$\mu\mu$ within the limits of accuracy of the observations. Already at that time the view had been advanced that the Nova had changed into a nebula, and had been rejected by me."

The numerous and complete observations of Campbell, at the Lick Observatory, are reviewed at length, the author dissenting however from Campbell's opinion that the spectrum of the Nova after its reappearance was nebular in character. Mr. Campbell's defense of his position has already been given in this journal (Oct., 1893) and hence no further comment seems necessary here than perhaps the remark that it is difficult to see how a stronger case could be made. Undoubtedly the Nova, if it had been observed at the time of its reappearance without any knowledge of its previous history, would have been classed as a planetary nebula. It is not necessary to conclude from this that a compact heavenly body suddenly changed into a gaseous nebula, for such a conclusion is derived from hypotheses which have yet to stand the test of critical examination, and perhaps of comparison with observations yet to be made.

Professor Vogel's picture of what must take place when a compact heavenly body of large mass enters a cosmical cloud, according to the hypothesis of Seeliger, is evidently not in accordance with mechanical principles. To assume, as he does, that the particles of the cloud rush upon the body from all sides after it is in their midst, is to neglect the attraction of the body before it enters the cloud. But the particles describe hyperbolic orbits around the body, their motion toward it begins at an indefinite period before it reaches them, and when they are rendered luminous they are moving in not greatly different directions. It would seem that Professor Seeliger's hypothesis, modified so as to give the compact body the unusual, but by no means impossible, velocity of 400 miles per second, in order to explain the absolute displacement of the dark lines in its spectrum, affords the most satisfactory explanation of the phenomena presented by the Nova that has yet been advanced.

A translation of Professor Seeliger's reply to these criticisms of his hypothesis will be given in our next number.

Sun Spots.—Dates of remarkable sun-spots:
Jan. 21st, 1892, Visible with naked eye.
Feb. 5th, 1892, " " " "
Feb. 10th, 1892, " " " "
Jan. 21st, 1893, Visible in opera-glass.
Feb. 5th, 1893, " " "
Feb 25th, 1893, " " "
Aug. 9th, cluster of spots plainly seen with naked eye.

Also, Aug. 19th and 30th, Sept. 3rd, 4th, 6th, and 28th. Oct. 12th to 19th, large spots were observed with my 4½-inch Clark telescope. Oct. 24th and 25th spots visible with naked eye. Dec. 7th and 8th spots plainly seen with field glass.

Brooklyn Village, Ohio, Dec. 11th, 1893. MARTIN WINGEY.

PHENOMENA DURING THE YEAR 1894.

H .C. WILSON.

Thinking that a general preview of the astronomical phenomena, which arto be expected during this year, would be of interest to our readers, we have prepared the following notes.

Eclipses.—There will be four eclipses, two of the Sun and two of the Moon, none of which will be of special interest. The first will be a ***partial eclipse of the Moon*** on March 21, beginning at $5^h\ 57^m$ A. M., and ending at $10^h\ 44^m$ A. M., central standard time. Only one-fourth of the Moon's diameter will be immersed in the Earth's shadow at the time of maximum eclipse.

The second will be an *annular eclipse of the Sun* beginning April 5 at $7^h\ 16^m$ P. M., and ending at $12^h\ 32^m$ A. M., April 6, central time. It will be visible as a partial eclipse in Asia, Alaska, and the eastern part of Europe. The path of the annular eclipse passes from the Indian Ocean across Hindostan, China, and Siberia, into Alaska.

The third will be a *partial eclipse of the Moon*, beginning Sept. 14 at $7^h\ 59^m$ P. M., and ending Sept. 15 at $1^h\ 04^m$ A. M. This will be visible throughout North and South America. The beginning will be visible in the western portions of Europe and Africa. Only 0.23 of the diameter of the Moon will be covered by the shadow at the middle of the eclipse.

The fourth will be a *total eclipse of the Sun*, beginning Sept. 28 at $9^h\ 1^m$ P. M. and ending Sept. 29 at $2^h\ 17^m$ A. M. central time. It will be visible mostly in inaccessible regions. The path of totality passes from Central Africa across the Indian Ocean to the south of Australia. As a partial eclipse it will be visible in the eastern part of Africa, Persia, Hindostan, the Indian Ocean, the southern part of Australia and one of the islands of New Zealand.

Transit of Mercury.—The planet Mercury will pass directly between the Earth and Sun on Nov. 10, so that for over five hours it will be seen projected as a round black spot upon the disc of the Sun. The transit will begin at $9^h\ 55^m$ A. M., and end at $3^h\ 12^m$ P. M. central time. The accompanying diagram, Fig. 1, will indicate the course which Mercury is to take across the solar disc. This transit, which will be the last to occur during this century, will be visible throughout North and South America, and in the western parts of Europe and Africa. Before November we will give the necessary data for computing the times of the contacts for different localities.

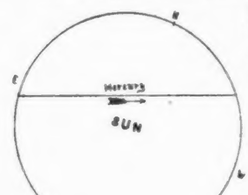


FIG. 1.

Occultations.—The usual number of occultations of stars by the Moon is to be expected. The lists of these for each month will be given one month in advance. We can, however, give only the data which apply to the occultations as seen from Washington, since these data are quite different for different localities, and the labor of calculating them for a sufficient number of points to make a general table even for the United States would be more than we can undertake. The Washington times will serve to call attention to the phenomena, but may be ex-

pected to be in error by many minutes, besides the difference in longitude for other places.

The Planets.—We have had the diagrams, Figs. 2 and 3, made in order to place before the eye of the reader the planets in their true places in their orbits, and relative positions with reference to the Earth and Sun. The circles represent the orbits of the planets, and their positions at the beginning and end of the year, and in some cases at the beginning of each month, are marked upon the circles. It was impracticable to draw them all to the same scale, because of the enormous dimensions of the orbits of Uranus and Neptune.

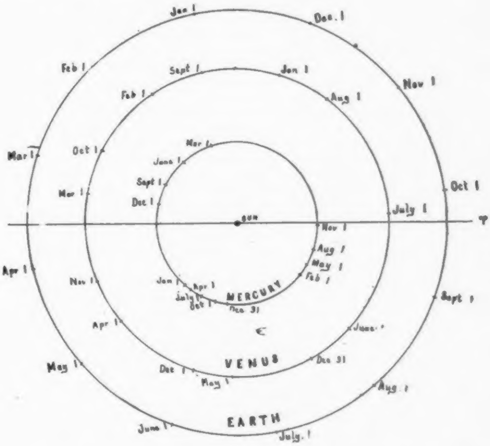


FIG. 2.—DIAGRAM SHOWING THE PLACES OF MERCURY, VENUS AND EARTH IN THEIR ORBITS DURING 1894.

At a glance one will see that Mercury makes a little more than four revolutions about the Sun, being seen from the Earth first on one side then on the other of the Sun. On Jan. 1 this planet as seen from the Earth, is to the right or west of the Sun. Toward the end of the month it will be behind the Sun, at superior conjunction. A few days before March 1 it will be at its greatest distance to the left or east of the Sun, and will therefore be visible in the evening just after sunset. About the middle of March Mercury will be in line between Earth and Sun, *i. e.*, at inferior conjunction, and invisible. A little study of the chart will show that we ought to expect to see Mercury as "evening star" in the latter part of February, the latter part of June and the middle of October, and as "morning star" about the middle of April, the first of August and the last of December.

In the same way we find that Venus will be "evening star" during January, but will pass between Earth and Sun in February, and after that will be "morning star," reaching her greatest distance to the right or west from the Sun in May. In November she will pass behind the Sun, becoming evening star again.

From Fig. 3 we see that Mars is just coming out from behind the Sun, and will not be in a very good position for three or four months yet, but that from July to the end of the year its position will be very favorable. Jupiter will be too close to the Sun for observation during May and June, and will be in best position in December. Saturn will be best seen in April and May, and will be invisible in

October. Uranus will be in best position in May and June, and Neptune in November and December. At the present time Mars, Saturn and Uranus are in that part of the sky which is visible in the morning, Jupiter and Neptune in the opposite region which is visible in the evening. Mars is rapidly leaving his companions behind, and at the end of the year will join Neptune and Jupiter.

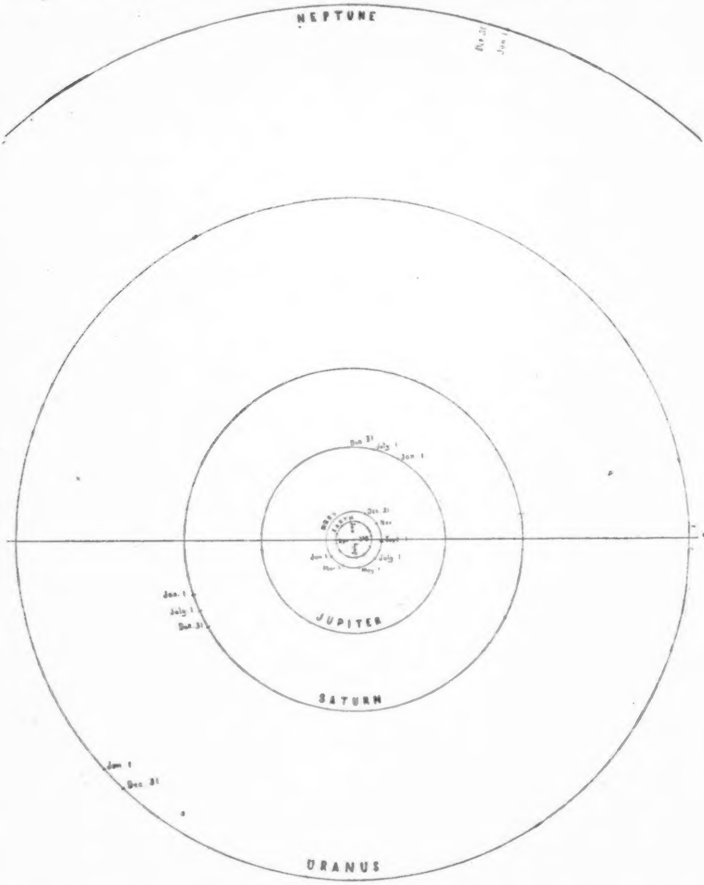


FIG. 3.—DIAGRAM SHOWING THE PLACES OF EARTH AND THE OUTER PLANETS IN THEIR ORBITS DURING 1894.

The chart, Fig. 4, shows the apparent paths which will be traced among the stars during the year by the planets Uranus and Saturn, and will enable the reader who is familiar with the constellations to identify the planets at any time.

Comets.—What new comets will be discovered of course we cannot predict, but two comets of short period are expected to return this year. The first, Tem-

pel II, the second comet of 1873, is due at perihelion April 20, and will be in good position for observation for several months after that time. It will be a telescopic comet. It was last seen in 1878, when it was observed for five months.

The second is Encke's periodic comet, which is not due at perihelion until Feb. 1895, but will be in best position for observation in December, 1894. This is also a telescopic comet, having a period of 3.3 years.

If the reader will refer to the plate of the Jupiter family of comets in the November number of ASTRONOMY AND ASTRO-PHYSICS he will see the orbits of these comets and their relation to that of the Earth.

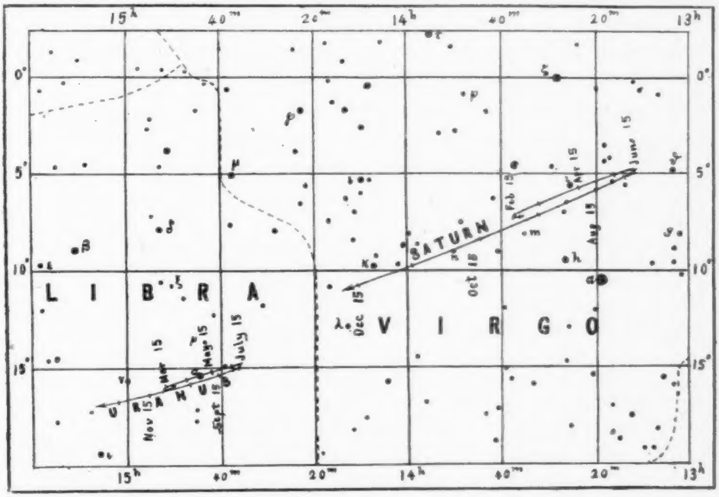


FIG. 4.—CHART SHOWING THE APPARENT PATHS OF SATURN AND URANUS AMONG THE STARS IN 1894.

The Phenomena of the Satellites.—The satellites of Mars are so small and faint that they are not likely to be seen by many amateurs. The most favorable time to make the attempt to see them will be in October when Mars is nearest the Earth.

The phenomena of Jupiter's four outer Satellites may be observed with a very moderate telescope. We will therefore give for each month the times of those phenomena which will be visible in the United States, and the configuration of the satellites at the most convenient hour for observation on each night. The diagram, Fig. 5, shows the apparent courses of the satellites around the planet for this year. The diagram gives the appearance seen in an inverting telescope. The vertical scale is made three times that of the horizontal scale in order to clearly separate the lines. Unfortunately the arrows indicating the direction of motion have been omitted from the cut. It is easy, however, to remember that the motion in the upper half of each satellite orbit is toward the left, and in the lower half toward the right. It will be noticed that all the satellites except IV pass in front of the planet when going toward the left, and behind it when moving toward the right. Satellite IV barely skirts the upper and lower edges of the planet. The time when a satellite enters upon the right edge of the disc of

Jupiter is designated in the table on page 78 as *Tr. In.* (transit ingress); that when it leaves the left edge *Tr. Eg.* (transit egress). The time when the satellite coming from the left goes behind the planet is designated *Oc. Dis.* (occultation disappearance); when it emerges on the right *Oc. Re.* (occultation reappearance). When the satellite enters the shadow of the planet the designation is *Ec. Dis.* (eclipse disappearance); when it emerges from the shadow, *Ec. Re.* (eclipse reappearance). The shadow of the planet as seen from the Earth is sometimes projected toward the right, sometimes toward the left, and sometimes directly behind the planet, according to the position of the Earth with reference to the line passing through the Sun and Jupiter, so that the last mentioned phenomena occur in different apparent positions with reference to the planet at different times. In February the shadow of Jupiter will be projected toward the right, so that the eclipses all occur after the occultations and on the right side of the planet.

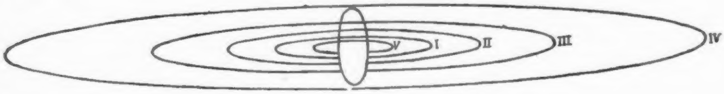


FIG. 5.—DIAGRAM SHOWING THE APPARENT COURSES OF THE SATELLITES OF JUPITER AROUND THE PLANET IN 1894.

The four diagrams at the top of page 77, marked *Phases of the Eclipses, etc.*, show where the observer should look for the disappearance and reappearance of each satellite. Satellite I is so near the planet that it enters the shadow while behind the planet, but reappears at the point marked r. Satellite II disappears in the shadow at r very soon after emerging from occultation, and reappears at r. The third satellite is so far out that both the disappearance and reappearance of eclipses occur quite a distance to the right of the planet, and IV does not enter the shadow at all. When the shadow of a satellite crosses the disc of the planet it is seen as a round black spot entering on the right and passing off the left edge. The beginning of this phenomenon is designated *Sh. In.* (transit of shadow ingress), and the end *Sh. Eg.* (transit of shadow egress).

The configuration of Jupiter's satellites will be indicated, as for February on page 77, for a given hour of each night, the light disc representing the planet and the dots the relative positions of the satellites. The numerals indicate the numbers of the satellites, and also the direction of their motions. The latter is always from the dot toward the numeral. A light disc at the left side of the page indicates that the satellite, whose numeral is attached, is projected upon the face of the planet; a black disc on the right, that the satellite is invisible by occultation or eclipse.

Five of the satellites of Saturn are usually visible with a telescope of moderate power. As they are best seen when at their greatest distances to the right or left of the planet (elongations), we will give each month the times of the eastern elongations. The western elongations will occur just half way between the eastern; and the positions of the satellites at other times can be interpolated with the aid of the diagram Fig. 6.

By this diagram the reader will see that the satellites of Saturn like those of Jupiter move toward the left in the upper half of their orbits and toward the right in the lower. Only the inner two transit across the face of the planet and are occulted. They are so small that they cannot be seen in transit, nor can

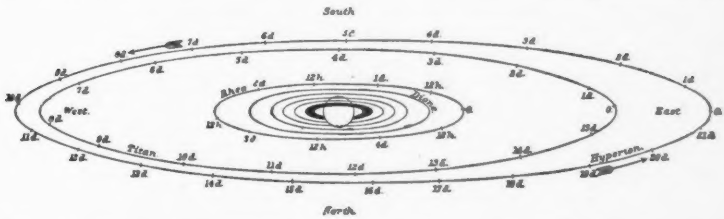


FIG. 6.—DIAGRAM SHOWING THE APPARENT COURSES OF THE SATELLITES OF SATURN AROUND THE PLANET IN 1894.

their shadows be seen. The positions of the satellites at intervals of one day, beginning with eastern elongation, are marked upon the diagram by the letters 1*d*, 2*d*, etc. The use of these will be readily seen from the following example: On Feb. 8 according to the table on page 78, Titan will be at eastern elongation at $12^h.9$ P. M. On the next day at the same time the satellite will be at the point marked 1*d*; at midnight of the 9th it will be midway between 1*d* and 2*d*, etc. The orbit of Iapetus is of the same form as the others but has a little more than twice the diameter of that of Hyperion.

The rings of Saturn will be in good position for observation this year. Their appearance is roughly indicated in the diagram.

The satellites of Uranus and Neptune are too faint to be seen except with large telescopes.

PLANET NOTES FOR FEBRUARY.

Mercury will be "evening star" during February. During the first half of the month he will be close to the Sun, but in the latter part will be visible to the naked eye for a short time after sunset. He will be at greatest elongation, east from the Sun 18°, on the evening of Feb. 25. His greatest brilliancy will be attained on the evening of Feb. 21. Mercury will be ten degrees due south from Venus at $9^h\ 41^m$ P. M. Feb. 8, central time.

Venus will be visible as evening planet for but a few days in February. On the 16th, at $3^h\ 04^m$ A. M., she will be at inferior conjunction, *i. e.*, between the Earth and Sun. Venus will be in conjunction with the crescent Moon, 11° north of the latter, at $3^h\ 03^m$ P. M. Feb. 6.

Mars will be visible in the southeast after 4^h A. M., but at too low an altitude for good observations in our latitude.

Jupiter will be at quadrature, 90° east from the Sun, Feb. 11, at $1^h\ 52^m$ A. M. He will be in excellent position for observation during the early part of the night. Jupiter will be in conjunction with the Moon, 4° 24′ north of the latter, Feb. 13 at $3^h\ 16^m$ A. M.

Saturn may be observed after midnight. Look toward the southeast in the constellation Virgo, about 5° northeast from the star Spica. The rings of the planet are easily seen with quite a small telescope. They are now turned at an angle of 14° to the line of sight, so that with telescopes of moderate power the divisions may be seen. Saturn's apparent motion among the stars during February will be westward. He will be in conjunction with the Moon, 4° north, at $8^h\ 02^m$ P. M. Feb. 23.

Uranus rises about midnight, and is in position for observation from 3 to 6 A. M. He is in the constellation Libra, about 1° 45′ east and 26′ south of the star α. Uranus will be at quadrature, 90° west from the Sun, Feb. 3 at $7^h\ 04^m$ P. M. He will be stationary in right ascension Feb. 18, and after that will move slowly westward. He will be in conjunction with the Moon, 3° 36′ north, at 9^h 58^m A. M. Feb. 25.

Neptune will be at quadrature, 90° east from the Sun, Feb. 29 at $2^h\ 36^m$ A. M. He will be in good position for observation during February. He is almost stationary in Taurus, a little more than one-third of the way on a straight line from ι to ε Tauri. There is no star of equal brightness, *i. e.*, 8th magnitude, within a radius of 1°.

Planet Tables for February.

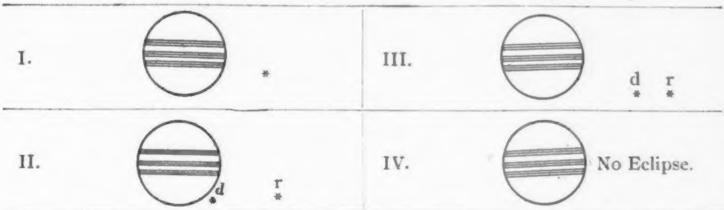
Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
			MERCURY.		
Feb. 5	21 41.7	− 15 51	7 41 A. M.	12 38.1 P. M.	5 35 P. M.
15	22 49.2	− 8 18	7 36 "	1 06.1 "	6 36 "
25	23 40.4	− 0 37	7 17 "	1 17.9 "	7 19 "
			VENUS.		
Feb. 5	22 11.8	− 3 43	7 19 A. M.	1 08.3 P. M.	6 57 P. M
15	21 49.5	− 4 12	6 20 "	12 06.7 "	5 54 "
25	21 28.2	− 6 00	5 26 "	11 06.2 A. M.	4 46 "
			MARS.		
Feb. 5	17 35.5	− 23 28	4 09 A. M.	8 32.6 A. M.	12 56 P. M.
15	18 05.8	− 23 43	4 01 "	8 23.5 "	12 46 "
25	18 36.2	− 23 37	3 52 "	8 14.6 "	12 37 "
			JUPITER.		
Feb. 5	3 20.0	+ 17 34	11 00 A. M.	6 15.6 P. M.	1 32 A. M.
15	3 23.4	+ 17 49	10 22 "	5 39.6 "	12 57 "
25	3 28.0	+ 18 08	9 46 "	5 04.9 "	12 24 "
			SATURN.		
Feb. 5	13 37.0	− 7 19	10 56 P. M.	4 30.8 A. M.	10 05 A. M.
15	13 36.5	− 7 13	10 16 "	3 51.0 "	9 26 "
25	13 35.4	− 7 04	9 35 "	3 10.5 "	8 46 "
			URANUS.		
Feb. 5	14 51.7	− 16 02	12 51 A. M.	5 49.2 A. M.	11 47 A. M.
15	14 52.0	− 16 04	12 12 "	5 10.1 "	10 08 "
25	14 51.9	− 16 03	11 33 P. M.	4 30.7 "	9 28 "
			NEPTUNE.		
Feb. 5	4 37.8	+ 20 34	12 03 P. M.	7 33.0 P. M.	3 03 A. M.
15	4 37.6	+ 20 34	11 23 "	6 53.5 "	2 24 "
25	4 37.6	+ 20 35	10 44 "	6 14.2 "	1 44 "
			THE SUN.		
Feb. 5	21 17.8	− 15 45	7 15 A. M.	12 14.3 P. M.	5 14 P. M.
15	21 57.3	− 12 29	7 01 "	12 14.3 "	5 28 "
25	22 35.5	− 8 53	6 45 "	12 13.2 "	4 41 "

Phases and Aspects of the Moon.

		d	*Central Time.* h m
Apogee	Feb.	1	4 00 P. M.
New Moon	"	5	3 45 P. M.
First Quarter	"	13	4 43 A. M.
Perigee	"	17	3 18 P. M.
Full Moon	"	19	8 17 P. M.
Last Quarter	"	27	6 28 A. M.

Jupiter's Satellites for February.

Phases of the Eclipses of the Satellites for an Inverting Telescope.



Configuration at 8h for an Inverting Telescope.

Day.	West		East.
1	3·	○	·1 2· ·4
2	.3 2· ·1	○	·4
3	·3 ·2	○	1· 4·
4	·1	○	·3 ·2 4·
5		○	1·2· 4· ·3
6	2.	○	4· 3. ·1 ●
7	4· 1·	○	3· ·2 ●
8	4· 3·	○	·1 2·
9	4· ·3 1·2·	○	·
10	4· ·3 ·2	○	1·
11	·4 ·1	○	·2 ·3 ●
12	·4	○	1· 2· ·3
13	·4 2·	○	3· ·1 ●
14	·4 1··2	○	3·
15	3·	○	·4 ·1 2·
16	3· 1· 2·	○	·4
17	·3 ·2	○	·1 ·4
18	·1	○	·3 ·2 .4
19		○	1· 2· ·3 ·4
20	2· ·1	○	·3 4·
21	○ 1· ·2	○	3· 4·
22	3·	○	·1 ·2 4·
23	○ 2· 3· 1·	○	4·
24	·3 ·2 4·	○	·1
25	4· ·1 ·3	○	·2
26	4·	○	1· 2· ·3
27	4· 2· .1	○	3·
28	·4 ·2	○	1· 3·

Occultations Visible at Washington.

Date 1894.	Star's Name.	Magnitude.	IMMERSION Washington M. T.		IMMERSION Angle f'm N pt.	EMERSION Washington M. T.		EMERSION Angle f'm N pt.	Duration.	
			h	m	°	h	m	°	h	m
Feb. 11	19 Arietis	6	6	49	76	8	02	226	1	13
12	ζ Arietis	5	11	13	97	12	07	237	0	54
16	c Geminorum	6	17	17	59	17	49	332	0	32
20	σ Leonis	4	9	55	152	10	58	277	1	03

Elongations of the Satellites of Saturn.

(The western elongations will be found approximately half way between the eastern and other positions may be easily interpolated.)

MIMAS.

		h		
Feb.	3	6.7	A. M.	W
	4	5.3	"	W
	5	3.9	"	W
	6	2.5	"	W
	7	1.1	"	W
	11	6 9	"	E
	12	5.5	"	E
	13	4.1	"	E
	14	2.8	"	E
	15	1.4	"	E
	15	midn.		E
	20	5.7	"	W
	21	4.3	"	W
	22	3.0	"	W
	23	1.6	"	W
	24	12.2	"	W
Mar.	1	4.6	"	E

ENCELADUS.

Feb.	2	2.4	P. M.	E
	3	11.3	"	E
	5	8.2	A. M	E
	6	5.1	P. M.	E
	8	1.9	A. M.	E
	9	10.8	"	E
	10	7.7	P. M.	E
	12	4.5	A. M.	E
	13	1.5	P. M.	E
	14	10.3	"	E
	16	7.2	A. M.	E
	17	4.1	P. M.	E
	19	1.0	A. M.	E

ENCELADUS CONT.

		h		
Feb.	20	9 9	A. M.	E
	21	6.7	P. M.	E
	23	3.6	A. M.	E
	24	12.5	P. M.	E
	25	9.4	"	E
	27	6.3	A. M.	E
	28	3.1	P. M.	E

TETHYS.

Feb.	2	4.4	A. M.	E
	4	1.7	"	E
	5	11.0	P. M.	E
	7	8.3	"	E
	9	5.6	"	E
	11	2.9	"	E
	13	12.2	"	E
	15	9.5	A. M.	E
	17	6.8	"	E
	19	4.1	"	E
	21	1.4	"	E
	22	10.7	P. M.	E
	24	8.0	"	E
	26	5.3	"	E
	28	2.6	"	E

DIONE.

Feb.	1	3.1	P. M.	E
	4	8.8	A. M.	E
	7	2.4	"	E
	9	8.1	P. M.	E
	12	1.8	"	E
	15	7.4	A. M.	E

DIONE CONT.

		h		
Feb.	18	1.1	A. M.	E
	20	6.8	P. M.	E
	23	12.5	"	E
	26	6.1	A. M.	E
	28	11.8	P. M.	E

RHEA.

Feb.	3	7.7	A. M.	E
	7	8.0	P. M.	E
	12	8.4	A. M.	E
	16	8.8	P. M.	E
	21	9.2	A. M.	E
	25	9.6	P. M.	E

TITAN.

Feb.	4	6.0	P. M.	S
	8	12.9	"	E
	12	10.5	A. M.	I
	16	2.0	P. M.	W
	20	4 7	"	S
	24	11.2	A. M.	E
	28	9.5	"	I

HYPERION.

Feb.	2	6 6	P. M.	I
	8	3.2	A. M.	W
	12	12.8	P. M.	S
	17	4.7	"	E
	24	12.9	A. M.	I
Mar.	1	9.2	"	W

IAPETUS.

Feb.	28	1.0	A. M	W
Mar.	19	9.9	P. M.	S

Phenomena of Jupiter's Satellites.

Central Time.

		h m			
Feb.	4	5 06	P. M.	III	Ec. Re.
		11 30	"	I	Oc. Dis.
	5	8 41	"	I	Tr. In.
		10 00	"	I	Sh. In.
		10 53	"	I	Tr. Eg.
		11 29	"	II	Tr. In.
	6	5 58	"	I	Oc. Dis.
		9 29	"	I	Ec. Re.
	7	5 22	"	I	Tr. Eg.
		5 49	"	II	Oc. Dis.
		6 42	"	I	Sh. Eg.
		8 13	"	II	Oc. Re.
		8 31	"	II	Ec. Dis.
		10 41	"	II	Ec. Re.
	9	5 49	"	II	Sh. Eg.
	11	7 25	"	III	Ec. Dis.
		9 09	"	III	Ec. Re
	12	10 36	"	I	Tr. In.
	13	7 54	"	I	Oc. Dis.
	14	6 24	"	I	Sh. In.
		7 18	"	I	Tr. Eg.
		8 25	"	II	Oc. Dis.
Feb.	14	8 37	P. M	I	Sh. Eg.
		10 49	"	II	Oc. Re.
		11 07	"	II	Ec. Dis.
	15	5 54	"	I	Ec. Re.
	16	5 51	"	II	Tr. Eg.
		6 05	"	II	Sh. In.
		8 27	"	II	Sh. Eg.
	18	5 54	"	III	Oc. Dis.
		7 59	"	III	Oc. Re.
	20	9 51	"	I	Oc. Dis.
	21	7 00	"	I	Tr. In.
		8 19	"	I	Sh. In.
		9 14	"	I	Tr. Eg.
		10 33	"	I	Sh. Eg.
	22	7 50	"	I	Ec. Re.
	23	6 07	"	II	Tr. In.
		8 31	"	II	Tr. Eg.
		8 43	"	II	Sh. In.
	25	5 18	"	II	Ec. Re.
		10 01	"	III	Oc. Dis.
	28	8 58	"	I	Tr. In.
		10 15	"	I	Sh. In.

Approximate Times when the Great Red Spot will pass the Central Meridian of Jupiter.

		h m			h m			h m
Feb.	2	1 20 A. M.	Feb.	11	11 39 P. M.	Feb.	21	9 58 P. M.
		9 12 P. M.		12	7 31 "		22	5 50 "
	4	10 51 "		14	1 18 A. M.		23	11 38 "
	5	6 42 "		14	9 10 P. M.		24	7 29 "
	7	12 30 A. M.		16	10 49 "		26	1 16 A. M.
		8 21 P. M.		17	6 40 "			9 08 P. M.
	9	2 09 A. M.		19	2 28 A. M.		28	2 56 A. M.
		10 00 P. M.			8 19 P. M.			10 47 P. M.
	10	5 52 "		21	2 06 A. M.			

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

R. A................$0^h\ 52^m\ 32^s$
Decl.................+81° 17′
Period............*2d* $11^h\ 50^m$

Feb.	1	9 A. M.
	6	8 "
	11	8 "
	16	8 "
	21	7 "
	26	7 "

ALGOL.

R. A...............$3^h\ 1^m\ 1^s$
Decl................+ 42° 32′
Period............*2d* $20^h\ 49^m$

Feb.	11	6 A. M.
	14	2 "
	16	11 P. M.
	19	8 "

R CANIS MAJORIS.

R. A.............. $7^h\ 14^m\ 30^s$
Decl........ ......— 16° 11′
Period........ ..*1d* $3^h\ 16^m$

Feb.	3	7 P. M.
	4	10 "
	6	2 A. M.
	7	5 "
	11	6 P. M.
	12	9 "
	13	midn.
	15	4 A. M.
	20	8 P. M.
	21	11 "
	23	2 A. M.
	24	6 A. M.
	28	7 P. M.

S CANCRI.

R. A............. $8^h\ 37^m\ 39^s$
Decl...............+ 19° 26′
Period............*9d* $11^h\ 38^m$

Feb.	6	3 A. M.
	25	2 "

S ANTLIÆ.

R. A............. $9^h\ 27^m\ 30^s$
Decl...............— 28° 09′
Period........... *0d* $7^h\ 47^m$

Feb.	1	8 P. M.
	2	4 A. M.
	2	7 P. M.
	3	3 A. M.
	3	7 P. M.
	4	2 A. M.
	5	2 "
	6	1 "
	6	midn.
	7	"
	8	11 P. M.
Feb.	9	10 P. M.
	10	10 "
	11	9 "
	12	8 "
	13	8 "
	14	3 A. M.
	14	7 P. M.
	15	3 A. M.
	16	2 "
	17	2 "
	18	1 "
	18	midn.
	19	midn.
	20	11 P. M.
	21	10 "

S ANTLIÆ CONT.

Feb.	22	10 P. M.
	23	9 "
	24	8 "
	25	8 "
	26	3 A. M.
	26	7 P. M.
	27	3 A. M.
	28	2 "

δ LIBRÆ.

R. A........... ..$14^h\ 55^m\ 06^s$
Decl.............. — 8° 05′
Period........... *2d* $07^h\ 51^m$

Feb.	8	7 A. M.
	15	6 "
	22	6 "

U CORONÆ.

R. A ....$15^h\ 13^m\ 43^s$
Decl.............. + 32° 03′
Period........... *3d* $10^h\ 51^m$

Feb.	7	9 P. M.
	14	7 "
	18	6 A. M.
	25	4 "

U OPHIUCHI.

R. A.............$17^h\ 10^m\ 56^s$
Decl.............. + 1° 20′
Period........... *0d* $20^h\ 08^m$

Feb.	3	6 A. M.
	4	2 "
	8	6 "
	9	3 "
	14	3 "
	19	4 "
	24	5 "
	25	1 "

New Asteroid 1893 AO.—This was discovered by Wolf at Heidelberg, on a photographic plate taken Nov. 6. Its position at $9^h\ 18^m$ Heidelberg M. T. was R. A. $2^h\ 19^m$; Decl. + 11° 22′. Daily motion — 0.6^m in R. A. and — 2′ in Decl. Magnitude 13.

Ephemeris of Comet *c* 1893 (Brooks).—From Professor Porter's last elements. I have computed the following ephemeris of Comet Brooks.

G. M. T.	App. R. A.			App. Dec.		Log *r*	Log Δ
1894	h	m	s	°	′		
Jan. 1.5	20	7	34	+75	12	0.2894	0.1864
2.5	20	23	7	74	56		
3.5	20	37	53	74	39		
4.5	20	51	57	74	19		
5.5	21	5	4	73	58	0.3009	0.2054
6.5	21	17	18	73	32		
7.5	21	28	53	73	7		
8.5	21	39	50	72	41		
9.5	21	50	2	72	14	0.3123	0.2255
10.5	21	59	20	71	46		
11.5	22	8	11	71	17		
12.5	22	16	34	70	49		
13.5	22	24	24	70	20	0.3234	0.2462
14.5	22	31	34	69	50		
15.5	22	38	24	69	21		
16.5	22	44	55	68	53		
17.5	22	51	4	68	24	0.3340	0.2672
18.5	22	56	46	67	56		
19.5	23	2	14	67	27		
20.5	23	7	28	66	59		
21.5	23	12	26	66	32	0.3444	0.2881
22.5	23	17	3	66	5		
23.5	23	21	30	65	39		
24.5	23	25	47	65	13		
25.5	23	29	54	64	48	0.3545	0.3088
26.5	23	33	50	64	23		
27.5	23	37	36	63	59		
28.5	23	41	13	63	35		
29.5	23	44	39	63	12	0.3642	0.3290
30.5	23	47	55	62	49		
31.5	23	51	1	+62	27		

The theoretical brightness of the comet is slowly diminishing.

O. C. WENDELL.

Harvard College Observatory, Dec. 16, 1893.

NEWS AND NOTES.

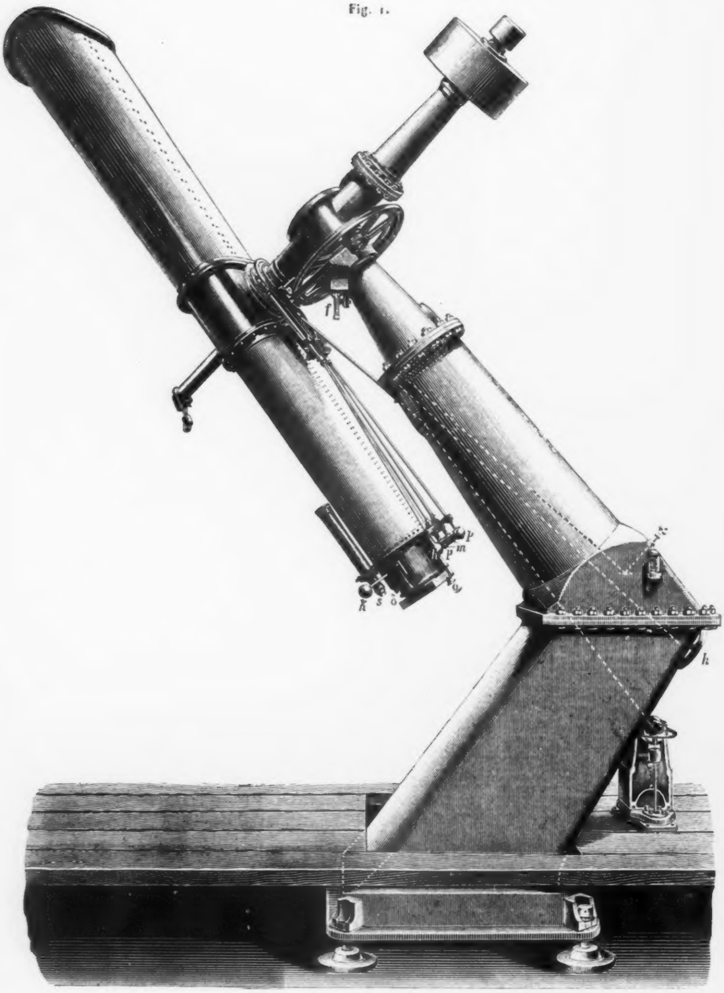
This issue is sent to a large number of subscribers who have not yet renewed for the coming year. In each will be found a printed slip reminding of this fact. The publisher respectfully asks that *every* one so notified will promptly signify whether or not the continuance of this publication is desired.

It is very much hoped that the subscription list for the coming year may be largely increased, in order to warrant the publisher in giving more and better illustration every month for the articles that need it. Will each subscriber interested enough to aid in this, send two names in the blank found in the first advertising page?

The paid subscription list for POPULAR ASTRONOMY, in its first four months, has outrun that of this publication, even in its enlarged form. This has been unexpected, and naturally leads the publisher to ask if ASTRONOMY AND ASTRO-PHYSICS is being conducted in the most useful and effective way. From the

PLATE V.

Fig. 1.



THE PHOTOGRAPHIC TELESCOPE OF THE POTSDAM OBSERVATORY.

varied and hearty commendations, received from leading scientists at home and abroad, during the last two years, the publisher has believed that this periodical was fairly entitled to rank with the first of its kind. That it has grown too technical and heavy for popular readers, students, and many amateurs, has been evident for more than a year. This was the reason why it seemed necessary to bring out another simpler publication of the same kind. It remains now to be seen whether or not there is really support enough for so expensive a publication as this. The coming year will doubtless answer this query.

Honors for Professors Hall and Barnard.—The December number of *L' Astronomie* conveys the information that at a recent meeting of the Astronomical Society in France, M. Tisserand, president, and director of the Paris Observatory, announced that the Academy of Sciences had decided to confer the Arago gold medal upon Professor Hall for his discovery of the Satellites of Mars, and upon Professor Barnard for his discovery of the fifth satellite of Jupiter, each medal to be worth 1,000 francs. The Arago medal has been conferred only once before, upon the illustrous Leverrier, in recognition of his discovery of Neptune, and is an honor that comes to very few astronomers. The discoveries of the satellites of Mars and of the fifth satellite of Jupiter are justly reckoned among the greatest observational achievements of this century, and we are gratified that the labors of such eminent astronomers as Professors Hall and Barnard are so fully appreciated in the highest scientific circles abroad. No higher tribute than the Arago medal could have been paid to American science, and the honor thus conferred upon Prof. Hall and the great astronomer at the Lick Observatory will be fully appreciated by all American astronomers.

The Photographic Telescope of the Potsdam Observatory.—The photographic telescope of the Potsdam Observatory is an ideal one so far as convenience and utility are concerned. The mounting may be said to be a cross between the German and the English forms. It has the good qualities of both these, and the bad qualities of neither. The tube it mounted on a polar axis that has no northern support. This pier, which is of iron and tapering, is bent at a certain point to an angle equal to the latitude. This bent portion is the polar axis upon which the tube and declination axis are mounted. The telescope is therefore free to move under the polar axis even when pointed to the north pole. It is thus possible to point the instrument to any part of the heavens and then revolve it through 360° without touching the pier or meeting with any obstruction to the view. An uninterrupted exposure can therefore be had of any object throughout its diurnal path.

The optical part of the telescope consists of two objectives mounted in a double tube. These glasses are 13 (French) and 9 (French) inches in diameter. The 9-inch is a visual, and is used as a guiding telescope for the 13-inch which is corrected for photography. Both objectives are of the same focus, viz., 3.4 meters. They were made by Steinheil, and were mounted by Repsold.

It might be suggested that this form of mounting would be subject to more or less flexure as there is no support for the northern end of the polar axis where the weight of the tube falls. Dr. Scheiner, however, states that it is the most stable instrument they have at Potsdam, and that it has not shown any displacement greater than 15″ in two years.

That the method is safe for instruments not exceeding 13 inches is shown by the fine photographs of clusters (especially M 5, M 11, and M 13), that have been made with it.

Every photograph made with this instrument has a *reseau* impressed upon it, so that accurate measures of the star positions may be made. This *reseau* is impressed on the plate before it is exposed to the stars. In securing an impression of the *reseau* the greatest care must be exercised to avoid errors of parallax, for it cannot be placed in contact with the sensitive plate. The method of doing this at Potsdam is as follows: A small electric lamp is placed in the focus of the 13-inch. Over the object glass is placed a tightly-fitting cap. This cap contains a plate holder with the sensitive plate in it. In a similar holder, between the object glass and the plate, is the *reseau* which consists of a plane glass plate on which an opaque film of silver has been deposited. Upon this the reticle has been ruled leaving lines of clear glass. The small electric light being in the focus of the telescope, its rays will leave the object-glass parallel and thus impress on the plate an image of the *reseau* without any errors of parallax.

Orbit of the Companion of Sirius.—In Gould's Astronomical Journal for Feb. 4th, 1891, under the title of "Orbit of the Companion of Sirius" I described a graphical method for finding the "best" orbit of a double star from "scattering" observations. This method seems to me to be in one respect superior to that described by Professor See, in his paper read before the congress of ASTRONOMY AND ASTRO-PHYSICS at Chicago and published in the last number of this journal, because after doing all that he describes, it takes an additional step in the direction of securing an orbit to give a still better general average of all the observed positions.

For brevity I will only here refer directly to the illustration of the orbit of γ Virginis accompanying Professor See's paper. I am quite sure that if he had gone one step farther, and following the method above referred to had connected each platted observed position, by an arrow, with the place of the companion in its assumed orbit at the corresponding date; so that it could be seen at a glance whether the companion, moving according to the law of equal areas, was anywhere systematically either ahead of or behind its observed positions, a still better orbit would have been suggested. CHARLES P. HOWARD.

The editor has kindly submitted to me the above note of Mr. Howard, who raises a point not fully developed in my paper, but it is not one which has escaped my attention in actual work on Double Star Orbits. Mr. Howard seems to assume that I have not sufficiently tested the equality of the areas, and suggests a method for doing this which would be good except for the additional complication thus introduced into the diagram.

In determining orbits I have always measured up the areas by the method of small triangles used by Professor Burnham, which is in all respects satisfactory and has the advantage of leaving the diagram free from unnecessary complications.

It must not, however, be assumed that no systematic deviation should appear in the observed areas, for systematic errors in angle are often conspicuous, and hence the apparent ellipse should not satisfy these erroneous angles.

The final test of an orbit must be derived from a comparison of the computed with the observed angles and distances, but in this comparison, as in the graphical work, care must be exercised to exclude worthless observations.

T. J. J. SEE.

The British Photographs of the Recent Total Eclipse of the Sun.—The British Eclipse Expeditions for photographing the total solar eclipse of April 16, 1893,

have proved very successful, and a series of beautiful negatives of the corona were secured at the Brazilian and African stations.

The two stations were supplied with identical apparatus.

1st. A 4-inch photographic lens 60 inches focus.

2nd. A 4-inch lens of 60 inches focus with a meniscus enlarging lens of 8 inches negative focus, the total focal length being 5 feet, 6 inches. These instruments were the work of Dallmeyer. The latter instrument gave an image of the Sun 1½ inch in diameter, or an enlargement of about three times.

In Africa 9 negatives were secured altogether; four with the enlarging lens and five with the other instrument. These negatives were made by Sergeant J. Kearney, R. E.

In Brazil 12 negatives were obtained; six with the enlarging apparatus and six without enlargement. They were made by Mr. Taylor.

The plates used were the Cadet and the developer Pyro-Ammonia.

The exposure times were $20^s - 150^s - 50^s - 5^s - 2^s - 15^s$, in the order of exposure.

The best negatives were obtained with the shorter exposures. This was a necessary result, due to the fact that the corona is not photographed on a black sky; it must be discriminated from a partially luminous background, and this can best be done with reasonably short exposures, as was pointed out and insisted upon by Mr. Burnham in the preparation for the eclipse of Jan. 1, 1889, (see also p. 73, Lick Observatory Eclipse Report, Jan. 1, 1889).

The negatives from Brazil and Africa, show a splendid amount of detail and extension. The enlarged images are specially fine and show that henceforth large pictures of the corona may be secured with small instrumental equipment which can easily be transported to the most inaccessible places. Indeed Mr. Taylor is confident that a very much larger image could have been successfully secured with the same instrument and he would now prefer an enlargement of eight times instead of three as he thinks the results would have been equally successful on the greater scale. By all means the method of direct enlargement in the telescope should be used hereafter in all expeditions.

The careful development of these English eclipse negatives is specially to be commended, as not only the extensions are secured but also the details near the Moon are carefully preserved in the development. Some of the plates suffered somewhat from the warm, moist climate to which they were subjected.

The corona of 1893 as shown on these negatives is singularly different from that of 1889, Jan. 1st. or 1889, Dec. 22. There is an absence of the beautiful polar fans, and the great equatorial wings of which the coronas of those dates were characteristic. The corona of 1893 more nearly resembles that of 1871 in the distribution of the coronal streams in all latitudes.

One of the most important things to be settled by observations of this eclipse was the question whether rapid changes occur in the form of the corona. The photographs of 1889, Jan. 1, and 1889, Dec. 22, showed that great changes certainly took place in the interval of one year, but no successful photographs had ever been made of the same eclipse with sufficient interval in absolute time to show if these changes were great enough and rapid enough to be shown in a few hours' time. There was certainly no change in a few minutes. This question would perhaps have been settled at the eclipse of 1889, Dec. 22, had the observations not failed in Africa on account of cloudy weather.

In the present eclipse all the parties sent to secure photographs of the corona were very successful, and the stations Chile, Brazil and Africa were far enough

apart in point of actual time of totality to settle this question pretty definitely—at least so far as any considerable change was concerned—and the question has in all probability been settled definitely and in the negative, so far as to change in a few hours.

The negatives of the Brazilian and African expeditions are in the hands of Mr. W. H. Wesley, the secretary of the Royal Astronomical Society, for comparison.

Mr. Wesley's previous experience in such work coupled with his great artistic skill, makes him eminently qualified to conduct such an investigation. Very fortunately, through the courtesy of Professor W. H. Pickering, Mr. Wesley has also the opportunity to compare with these the original negatives of the Harvard party in Chile, which gives a still greater time interval.

Though it will require more time than Mr. Wesley has yet been able to give to the subject to settle definitely the question of change in these photographs, his comparisons have already been of the greatest interest as they prove that no great change can have taken place.

From the extreme interest attached to this very important question, it will perhaps be an excusable act to quote from a recent letter of Mr. Wesley's in answer to a question as to whether his investigations showed any change among the photographs. From a serious illness in his family, Mr. Wesley had not been able to give as much time to the work as he had wished.

"So far as I have gone" he writes, "I am by no means sure if there has been *any* change in the corona. If there is any it is *extremely* slight."

At first there seemed to be some changes but as all the photographs did not verify this they were supposed to be photographic effects.

Mr. Wesley says further:—

"The elimination of *apparent* differences (of a photographic nature) will, I see, be a very difficult task, and until I have made much more careful study of the Brazil and African photographs, I am not in a position to speak positively with regard to *real* changes in the corona. They can, however, only be very slight."

The more detailed investigation of these valuable photographs by Mr. Wesley will be watched for with the greatest interest.

Though no certain changes have so far been shown in the corona, considerable changes, as would be expected, were noticeable in the prominences in the negatives from the different stations.

The last negative of Mr. Taylor's list shows "Bailey's Beads" very beautifully where the Sun is just emerging.

The Motion of 61 Cygni.—The *Sitzungsberichte* of the Berlin Academy of Oct. 26, contains the announcement by Dr. J. Wilsing of an observed variation of short period in the distance of the components of 61 Cygni. A series of photographic plates for the determination of the parallax of this star was begun at Potsdam in the autumn of 1890. In the course of the reduction of these measures it was found that certain discordances existed between the parallaxes derived from different comparison stars. These discordances could not be accounted for on the ground of errors of observations, nor did the measures of the comparison stars themselves show evidence of a difference between their own respective parallaxes. Dr. Wilsing was therefore led to suspect the presence of one or more unknown bodies in the system, and to investigate by observation the effect upon the distance between the two visible components. Great care was taken to eliminate from the results all sources of error known to affect photographic observations, and the reductions were carried out with all necessary precision. The series of observations extended from 1890, October, to 1893, September. The following table contains the results suitably grouped in means. They have all been corrected to 1891.0 for proper motion by assuming that the continuous yearly increase of the distance is 0″.10.

Mean Date.		Observed Distance. ″	Prob. Error. ″	Reduced Distance. ″	Diff. from Mean.	No. of Plates.	Impressions.
1890	Oct. 18.........	20.97	± 0.042	20.99	+ 0.041	2	4
	Nov. 5........	20.89	.060	20.91	− .039	1	2
	Dec. 17.........	20.95	.060	20.95	+ .001	1	2
1891	Feb. 4........	20.99	.060	20.98	+ .031	1	2
	May 13...... ..	20.95	.024	20.91	− .039	6	12
	June 14........	20.82	.032	20.77	− .179	4	8
	Aug. 25........	21.08	.017	21.02	+ .071	12	24
	Sept. 17.......	21.05	.017	20.98	+ .031	12	24
	Oct. 13.........	21.14	.019	21.06	+ .111	10	19
	Nov. 11........	21.17	.022	21.08	+ .131	8	16
	Dec. 17.........	21.20	.021	21.10	+ .151	9	17
1892	Jan. 15.........	21.14	.023	21.04	+ .091	2	14
	May 16........	21.08	.022	20.94	− .009	5	15
	June 15........	21.11	.026	20.96	+ .011	4	11
1893	Jan. 13.........	21.14	.014	20.94	− .009	6	40
	Mch. 24........	21.01	.019	20.79	− .159	3	19
	Apr. 15........	21.01	.017	20.78	− .169	3	24
	May 14........	21.10	.015	20.86	− .089	4	34
	June 11........	21.14	.016	20.90	− .049	4	32
	July 18.........	21.22	.017	20.97	+ .021	3	24
	Aug. 15........	21.22	.017	20.96	+ .011	3	25
	Sept. 8.........	21.25	.020	20.98	+ .031	2	18

A comparison of the fifth column of the above table with the third shows that the systematic differences are too large in comparison with their probable errors to be taken as the result of errors of observation. Dr. Wilsing gives also a graphical representation of the observations by means of a curve. He concludes finally that the distance of the two visible components of 61 Cygni is subject to a fluctuation as great as 0″.3, and having a period not far from 22 months. Systems of this kind, he thinks, offer a connecting link between the spectroscopic and visual binary systems.

H. J.

Observatory at Manila, Philippine Islands.—It may be of interest to you and your readers to learn that the telescope which has been building for our Observatory at Manila has been completed and is now on its way to the Philippine Islands. A cut is now being made of this instrument which will be forwarded to you, as I believe it will be of interest to your readers, as the latitude of our Observatory is only 14° 22′ north.

The objective of this telescope is by Merz of Munich and is of the same size as the one in Strassburg and the one Schiaparelli is using at Milan, having an aperture of a little more than 18 Paris inches, being nearly 20 inches English.

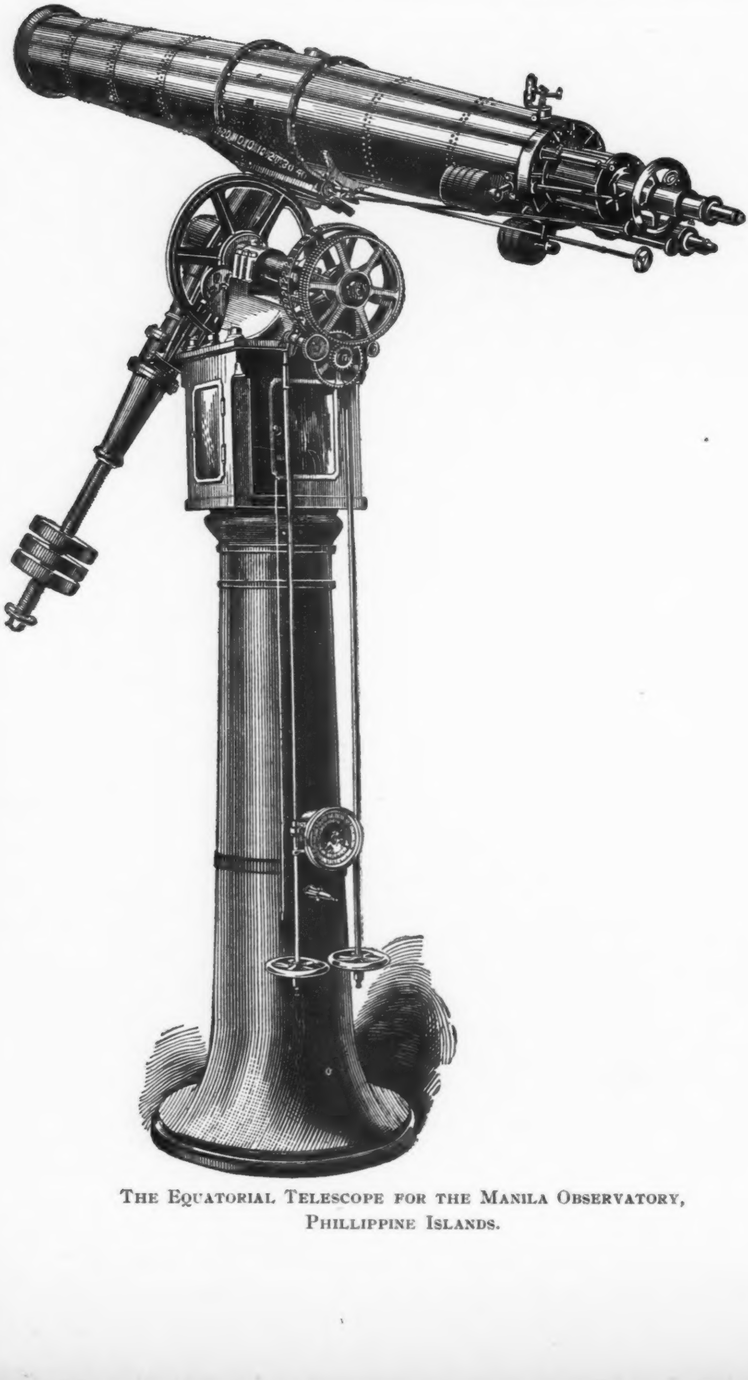
The instrument is very rigidly built, although mere weight was not the object sought after. The telescope tube weighs about one ton and about 5,000 pounds are being moved when the instrument is turned in R. A. It can be set in Declination and R. A. from the floor by means of two handwheels and finding circles, the hour circle being driven by a sidereal clock in order to be able to set directly to right ascensions.

The force necessary to move the telescope by means of the hand wheels is about 4 pounds on a radius of 14 inches. The motions are also communicated to the eye-end and it takes only a force of about 2 ounces to clamp and move the telescope either in R. A. or Declination. The fine hour circle can be read from the floor and the declination circle from the eye-end of the telescope. This eye-end is so arranged that the spectrograph and photographic apparatus can readily be attached; its construction is clearly shown in the cut.

The driving clock runs for over four hours with one winding and is provided with electric control. The illumination is by means of incandescent lamps and in addition, there is a self-adjusting oil lamp.

In design, execution and especially cost it compares most favorably with large telescopes of recent manufacture and it was finished in the short contract time of 10 months.

During the time it was mounted it was examined by the astronomers of the Naval Observatory, by those of Georgetown College and the Catholic University as well as by many scientists, all of whom expressed their admiration for the instrument.



The Equatorial Telescope for the Manila Observatory,
Phillippine Islands.

It was designed and built, as well as the other instruments for the Manila Observatory, already mentioned in a former number of this journal, by Geo. N. Saegmuller, Washington, D. C.

I hope soon to be able to give you more news of this instrument.

Georgetown College, Georgetown, D. C., JOSÉ ALGUÉ, S. J.

Nov. 25, 1893.

Auxilliary Quantities for Computing Precession.—The following tables taken from *Astronomische Nachrichten*, No. 3197, will be found very useful in reducing star places from the various epochs of the star catalogues in common use to 1894 and 1895. The table was prepared by Dr. H. Kreutz of Kiel, Germany.

Auxilliary Quantities for Computing Precession According to Struve for Several Common Epochs:

	$t = 1894.0$				$t = 1895.0$		
t_0	$m^s(t-t_0)$	$\lg[n^s(t-t_0)]$	$\lg[n''(t-t_0)]$	t_0	$m^s(t-t_0)$	$\lg[n^s(t-t_0)]$	$\lg[n''(t-t_0)]$
	m s				m s		
1790	+ 5 19.448	2.143209	3.319300	1790	+ 5 22.251	2.147364	3.323455
1800	+ 4 48.741	2.099295	3.275386	1800	+ 4 51.813	2.103890	3.279981
1810	+ 4 18.032	2.050437	3.226528	1810	+ 4 21.105	2.055576	3.231667
1825	+ 3 31.964	1.964993	3.141084	1825	+ 3 35.037	1.971241	3.147332
1830	+ 3 16.608	1.932319	3.108410	1830	+ 3 19.681	1.939051	3.115142
1835	+ 3 1.251	1.896986	3.073077	1835	+ 3 4.324	1.904284	3.080375
1836	+ 2 58.180	1.889561	3.065652	1836	+ 3 1.252	1.896985	3.073076
1840	+ 2 45.893	1.858524	3.034615	1840	+ 2 48.966	1.866492	3.042583
1842	+ 2 39.750	1.842131	3.018222	1842	+ 2 42.822	1.850403	3.026494
1845	+ 2 30.535	1.816321	2.992412	1845	+ 2 33.608	1.825094	3.001185
1850	+ 2 15.176	1.76957	2.94566	1850	+ 2 18.249	1.77933	2.95542
1855	+ 1 59.817	1.71718	2.89327	1855	+ 2 2.890	1.72818	2.90427
1860	+ 1 44.458	1.65759	2.83368	1860	+ 1 47.530	1.67018	2.84627
1864	+ 1 32.170	1.60323	2.77932	1864	+ 1 35.242	1.61747	2.79356
1865	+ 1 29.098	1.58851	2.76460	1865	+ 1 32.170	1.60323	2.77932
1870	+ 1 13.737	1.50631	2.68240	1870	+ 1 16.810	1.52404	2.70013
1872	+ 1 7.593	1.46852	2.64461	1872	+ 1 10.665	1.48783	2.66392
1875	+ 0 58.376	1.40485	2.58094	1875	+ 1 1.449	1.42713	2.60322
1880	+ 0 43.015	1.27222	2.44831	1880	+ 0 46.087	1.30218	2.47827
1885	+ 0 27.653	1.08033	2.25642	1885	+ 0 30.725	1.12609	2.30218
1890	+ 0 12.290	0.72814	1.90423	1890	+ 0 15.363	0.82505	2.00114
1895	− 0 3.073	0.12608*n*	1.30217*n*	1895	—	—	—
1900	− 0 18.436	0.90423*n*	2.08032*n*	1900	− 0 15.363	0.82504*n*	2.00113*n*

m and *n* are Struve's constants for the epoch $\frac{1}{2}(t+t_0)$.

If α' and δ' represent the approximate place of the star for the epoch $\frac{1}{2}(t+t_0)$, we have for the formulæ to be used:

$$\alpha = \alpha_0 + [m^s(t-t_0)] + [n^s(t-t_0)] \sin \alpha' \operatorname{tg} \delta'$$
$$\delta = \delta_0 + [n''(t-t_0)] \cos \alpha'$$

The Chicago Academy of Sciences—Section of Mathematics and Astronomy. Dec. 5th.—Professor G. W. Hough, president in the chair, after routine business had been transacted, the chair introduced Professor Albert A. Michelson of the University of Chicago, who read the paper of the evening on his "Determination of the Length of the Standard Meter in Terms of the Wave Length of the Light of Cadmium," recently made in France under the auspices of the International Committee of Weights and Measures.

Professor Michelson gave a résumé of the efforts hitherto made to find an absolute or natural standard of length, such as $\frac{1}{10000000}$ part of a quadrant of the

Earth's Meridian, and the length of a pendulum vibrating in seconds, and pointing out the practical difficulties which had rendered these efforts futile. He then discussed the history of the standard which Physicists had sought to base upon the wave-length of light as the unit, and proceeded to give an account of his own researches by means of the refractometer. This delicate instrument supplies a means of finding the number of light waves in a given small unit of length. The speaker said that experiments on the spectra of different elements had shown that the red line of cadmium was one of the sharpest and best fitted for supplying the desired light, and hence the cadmium line had been adopted in the experiments on the length of the meter. Professor Michelson then proceeded to give a full exposition of the theory of the Refractometer and to show its extreme accuracy. He gave an explanation of the intermediate standards employed in ascertaining the length of the meter, and spoke of the great precautions taken to insure the highest degree of precision. He said his work would soon be published by the International Bureau in Paris; but as it had not yet appeared, he would communicate the preliminary results of two complete and independent sets of measures, which gave

$$1553163\lambda$$
$$1553164\lambda$$

for the length of the standard meter. These results were received by members of the Acadamy with great enthusiasm, and the distinguished physicist was warmly congratulated on his splendid achievement. On motion of the recorder a vote of thanks was unanimously tendered the speaker for his interesting paper.

In the discussion which followed Judge Ewell, Professor Hough, Dr. Crew, Professor Burnham, Dr. Loves, and several other gentlemen took part. In conclusion, Professor Michelson exhibited a Refractometer and gave the members of the Academy an opportunity to observe some interesting interference phenomena.

T. J. J. SEE, Recorder.

New York Academy of Sciences, Section of Astronomy and Physics, Minutes of the meeting December 18, 1893.—The meeting was called to order at 8:15 P. M., with Professor Rees in the chair. The minutes of the previous meeting were read and approved.

Mr. Harold Jacoby presented the following report on the meeting of the National Academy of Sciences:

The National Academy of Sciences met in the Capitol at Albany, November 7-9. The papers presented included one by Dr. S. C. Chandler, entitled "Additional Researches on the Motion of the Earth's Pole." Dr. Chandler finds that the most recent observations obtainable (some still unpublished) confirm the doubly periodic law deduced by him. He showed that the two separate motions of the pole both take place from west to east. Dr. Chandler's paper was discussed by Professors Hall, Newcomb and Boss. They all expressed themselves as now favoring the truth of Dr. Chandler's law of variations. Professor C. S. Hastings read a paper on "A new form of telescopic objective, as applied to the twelve-inch equatorial of the Dudley Observatory." The principal characteristics of this instrument are: first, that one of the "ghosts" is made to coincide with the focal plane, thus rendering it harmless; and second, that the transformation from a visual to a photographic telescope is accomplished by substituting a second glass for one of the lenses of the visual combination, instead of adding a third lens. Professor Asaph Hall read a short paper on "Double Stars." Professor Charles L. Doolittle (introduced by Professor Boss) communicated a paper, "Latitude Determinations at the Sayre Observatory," but was unable to be present in person.

During the afternoon of November 8, the members of the Academy visited the new Dudley Observatory by invitation. The completed Observatory was opened for inspection, and an address was made by Professor Simon Newcomb.

Mr. Harold Jacoby also read:

"Some recent papers on the reduction of astronomical photographs." This paper will be contributed to the Astronomical Journal.

Professor Rees made some remarks on the photographic chart of the heavens.

Mr. Post exhibited a number of plates of the Pleiades and β Cygni which he had made at his Observatory at Bayport, L. I. He intended to measure these plates, in order to compare their accuracy with that attained by Rutherfurd and other astronomers.

HAROLD JACOBY,
Secretary of Section.